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ANNEX

THE MONIST

A QUARTERLY MAGAZINE

DEVOTED TO THE PHILOSOPHY OF SCIENCE

VOLUME XXVII

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VOL. XXVII. JANUARY, 1917. NO. 1

THE MONIST

HERMANN GRASSMANN.

1809-1877.

WE like to believe that the final significance of any thinker's work is independent of his time and place and is fixed by reference to some absolute standard. However that may be, it seems quite clear that his importance in his own age, and hence his effect on the next succeeding generations, depends to some extent on other factors than his intrinsic value. And so in judging that value we must distinguish plainly between it and what we might call the relative or historical importance of the man's work. This latter may well be compared to the potential of a body in electrostatics. For just as that potential depends not only on the actual charge on the body, but also on the charges on neighboring bodies; so also the relative importance of a man is not determined alone by the content of his life and work, but is affected also by his *milieu* and by the reactions of that *milieu* to it.

This is the reason why the contemporary estimate of a thinker is often so utterly wrong. At the time, the external man and his work are more easily seen; but the subtle tendencies of the age are not so readily understood, nor can the observer escape the distortion of vision wrought by prevailing influences on himself. So it comes about that he who is written down a failure in one age may stand out a very genius in the next.

These reflections are quite pertinent to any inquiry into the life and work of the author of the *Ausdehnungslehre*—Hermann Günther Grassmann, the distinguished mathematician whose own generation passed him by. Although he reached eminence in other branches of human activity, we speak of him as a mathematician because that was certainly the subject he loved most and in which his influence will be most felt in the future. Of him, on the occasion of his centenary (1909), F. Engel[1] could say: "To-day he is known by name to mathematicians, but few have read his writings. Even where his ideas and methods have been diffused in mathematical physics people learn them second-hand, sometimes not even under his name." So in Grassmann we have a straightforward example of a man between whose work and whose influence on his own and immediately succeeding generations we must sharply distinguish if we are to avoid underrating his significance.

He was born on April 15, 1809, in Stettin.[2] His father, Justus Günther Grassmann, was a teacher in the Gymnasium there, and was himself a good mathematician and physicist.[3] His school days passed without his showing any inclination or aptitude in special studies. He had however great skill in and fondness for music, and received a good foundation in piano and counterpoint from the famous composer Loewe. The latter was appointed teacher in the Stettin Gymnasium in 1820 and lived for the first year in the house of the Grassmanns, where he found very congenial society in Hermann and his brothers and sisters, all of whom were musical. With them Loewe often used to try over his new quartettes.

Of Grassman's inner development during these outwardly calm and uneventful years we can form a clear picture from his own writings. For in 1831 he wrote an account of his life in Latin in connection with the examination for his teacher's certificate; and later, in 1834, he handed in an autobiography to the Konsistorium in Stettin when he was passing his first theological examination. He refers to those earlier years as a period of slumber, his life being filled for the most part with idle reveries in which he himself occupied the central place. He says that he seemed incapable of mental application, and mentions especially his weakness of memory. He relates that his father used to say he would be contented if his son Hermann would be a gardener or artisan of some kind, provided he took up work he was fitted for and that he pursued it with honor and advantage to his fellow men. As he usually spent his holidays in the country among relatives, and nearly always in the families of clergymen, he conceived the desire to prepare himself for the ministry. But he soon came, partly from the ridicule of his companions and partly from the warnings of his parents, to doubt his capacity. He says however that, after his course of instruction for confirmation, a light came into his dreams. Suddenly he determined to exercise all his intellectual powers and to overcome as far as possible the phlegmatic character of his temperament. And this resolution he carried out with resistless energy.

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[1] F. Engel, Speech on "Grassmann in Berlin," to the Berliner Mathematische Gesellschaft (1909). To this I owe most of the information about Grassmann's early life given in what follows.

[2] The same date as Euler.

[3] He invented an air-pump cock which was given his name, and also constructed a useful index notation of crystals.

F. Engel[4] sums up these early years in the following words: "He does not belong to those early ripening geniuses who, even in childhood's years, know whither their gifts will lead them, and turn without doubt or hesitation to that branch of knowledge to which they are called. He was exceptionally gifted on too many sides for that. But even these many-sided gifts by no means showed themselves at the beginning; and that they developed themselves richly

[4] *Ibid.*

later came by no means without effort, but was the direct result of many years of concentrated work which he did in order to develop his character and to solidify his moral outlook and grasp of life."

In the August of 1827 Grassmann and his elder brother Gustav entered the University of Berlin with the intention of studying theology. Two days after their arrival Hermann wrote a droll letter to his mother vividly describing how they had settled in. He tells how they had to climb seventy-two steps to their attic dwelling at 53 Dorotheenstrasse at the corner of Friedrichstrasse. They had only room for their beds and two chairs, but he comments humorously on their extra fine look-out over the gardens and houses of the city, and adds that though the rooms were small they could be the more easily heated. His landlady will be recognized by students all the world over in his pen picture, "If she does talk too much, she is very pleasant and industrious." Particularly amusing is the manner in which he tells how they had spent practically all their money in two days. He enumerates all the possible and impossible things on which they had *not* spent the money, and finally confesses that their sudden impecuniosity was due to the piano which they luckily bought for 50 Taler.

Grassmann admitted later that when he first came to the university he was quite dependent on the guidance of the professors. He was easily impressed by the lectures he heard and tended to fit in his studies with the lectures he chanced upon rather than to take those corresponding to a course of study. At first he came specially under the influence of the well-known church historian Neander. Gradually, however, he became attracted more and more to Schleiermacher to whom he acknowledges great indebtedness. He wrote: "Early in my second year I attended Schleiermacher's lectures, which of course I did not understand; but his sermons began to exercise an influence upon me. However it was not until my third, and last, year that Schleiermacher entirely engaged my thought, and although at that time I was more occupied with philology, yet I then for the first time recognized how one could learn something from him for every branch of knowledge, because he aimed less at giving positive information than at making us capable of attacking each investigation in the right way and of carrying it on independently." From this we can see how Grassmann was coming to feel the joy of original creative work.

Though he had studied theology with his heart in his subject, he had by this time reached the decision to lay it aside. He says that he had noticed that clergymen who lived in country parishes, shut off from intercourse with scholars, lost grasp of their studies, however enthusiastic they had previously been, and ceased to pursue any investigation on their own account. To escape such a fate he decided to prepare himself as broadly as possible. For this reason he began the study of philology, but he continued it from sheer love of the subject. He had also made the discovery by this time that academic lectures are only of profit if taken in moderation; so he confined himself to two courses under Professor Boeckh, on the history of Greek literature and on Greek antiquities respectively. But he planned out a tremendous course of study, intending to begin with Greek grammar, then to read the Attic authors chiefly the historians, with the study of whom he would combine Greek history and antiquities—next the tragedians with mythology and poetic forms, and afterward Homer and Herodotus. Meanwhile he would seek variety by reading Roman authors. Finally, as he intended to follow his linguistic studies with mathematics, he meant to save Plato and Demosthenes until he began that study.

This exhaustive program he was not able to complete in Berlin. When he had reached the Attic authors he was taken ill in consequence of over-work. He describes his illness as neither severe nor dangerous, but it compelled him to slow down and to introduce more variety in order to avoid mental strain.

In this way he was led to the study of the sciences, but he showed his growing independence by working free of the schools. He did not attend a single mathematical lecture while a student in Berlin.

We may now see how wide his range of interests was throughout his university career. He seems to have been striving for as broad a foundation as possible, while at the same time he was building up a truly scientific attitude of mind which would enable him successfully to attack any subject he might turn his attention to. It is as though, as Engel says, he knew from the first that it would be necessary in his life to have more than one iron in the fire.

In the autumn of 1830 he returned to Stettin, and late in the following year took an examination for a teacher's certificate before the Scientific Examination Commission in Berlin. It was at this examination that he handed in the Latin autobiography we have previously referred to, and concerning which Köpke, rector of the monastary school of the Grey Friars in Berlin, comments *"Specimen tum propter rerum ubertatem tum propter stili venustatem et elegantiam laude dignum."* He was given permission to teach philology, history, mathematics, German and religious knowledge in lower and middle classes; but the commission at the same time expressed their expectation that he might easily perfect himself for teaching ancient languages and mathematics in all classes. This may have stimulated Grassmann to further mathematical studies, though he had already thrown himself with energy into them under the influence of his father, whose text-books he would naturally use.

He became assistant teacher (*Hilfslehrer*) in the Stettin gymnasium, and in 1832 began to lay the foundations of his great work, the "Theory of Extension" (*Ausdehnungslehre*). He began by working at the geometrical addition of straight lines, or what we now call vector addition. From this he was led to the notion of the geometrical product of straight lines. The direct influence of his father can best be shown by his own words:[5] "But I had not the slightest idea into what a rich and fruitful province I had here arrived; rather did this result[6] appear to me to be little worthy of notice until I combined it with a closely related idea. Namely, by following it up with the same idea of the product in geometry as my father had held,[7] it became evident to me that not only the rectangle but also the parallelogram in general may be considered as the product of two adjoining sides."

He goes on to add that he was surprised to find that he had thus reached a product which changed in sign if its factors were interchanged. And this, together with the fact that he was drawn into other spheres of work—one of which was the passing of the first theological examination at Stettin—caused this seed-idea to remain dormant for some considerable time.

In October, 1834, Grassmann returned to Berlin, this time as mathematics master in a trade school. Soon afterward he applied for a better position than the one he held and his principal gave the following characterization of him: "Mr. Grassmann is a young man not lacking in attainments. It is also apparent that he has given particular attention to the elements of mathematics, and thinks with especial clearness along that line, but he seems to have had little intercourse with people and is therefore backward in the usual forms of social life, shy, easily embarrassed and then very awkward. In the classroom all this vanishes when he does not know that he is observed. He then moves with ease, control, and certainty. In my presence, in spite of the fact that I have done all I could to give him confidence, he has not been able to become fully master of his embarrassment, which caused him much concern. My judgment of him is therefore as yet uncertain, and I cannot say whether he will be able suitably to fill the present vacancy."

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[5] Preface to the first edition of the 1844 *Ausdehnungslehre*.

[6] The notion of writing $AB + BC = AC$ whether the three points A, B, C are in the same straight line or not.

[7] Cf. J. G. Grassmann, *Raumlehre*, Part II, p. 164, and his *Trigonometrie*, p. 10.

As a matter of fact the vacancy was not an easy one to fill, since it had previously been held by no less a person than Jacob Steiner, the geometrician, who had been appointed to the university but retained some of the higher classes in geometry. Grassmann obtained the appointment; and as Steiner had bound himself to initiate his successor as far as possible into his own method of geometrical instruction, one would have expected interesting developments from the contact between the two men. There appears however to have been very little intimacy between them. There was a difference of thirteen years in their ages, and a wide contrast in temperament—the one self-reliant but thoroughly one-sided, the other diffident and many-sided. To these differences in personal characteristics Carl Müsebeck[8] is inclined to attribute their small effect on each other. Victor Schlegel's[9] view was that it was caused by the great difference in the methods employed by the two mathematicians. Whatever may have been the cause it is at any rate clear that Steiner's method of handling geometry had no influence whatever upon Grassmann's manner of thinking.

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[8] Carl Müsebeck, article on Hermann Grassmann, No. 3, Jahrgang 6 of the *Mathematisch-Naturwissenschaftliche Blätter*, p. 1, note.

[9] It is curious to note that V. Schlegel, who, as we shall see, was one of the first appreciators of Grassmann's work, long afterward used the methods of Grassmann's "Geometrical Analysis" to attack the problem of the minimum sum of the distances of a point from given points (*Bull. Amer. Math. Soc.*, Vol. I, 1894, p. 33) and reached a general result which reduces to Steiner's form of solution as a special case; thus illustrating the power of the method.

Several things combined to make Grassmann's stay in Berlin short. He was greatly distressed by the loss of his youngest sister, who was scarcely four years old, and this increased his inclination to religious brooding—to which he was the more inclined as he lacked suitable companionship. His eyesight also gave him some trouble, so that after a year and a quarter he gladly returned to Stettin on January 1, 1836, and became teacher in the Ottoschule.

He had, however, pleasant memories of these months in Berlin, as we can see from a letter written to his brother Robert, in which after speaking with pleasure of his return to Stettin he acknowledges the freedom and mental stimulation afforded by Berlin. At first glance this move from the capital seems a pity, since recognition of his talents might have come to him if he had stayed on. But we must remember to set against this, that he was very high-strung and energetic in mind and could be easily over-stimulated —an effect helped by the quiet life he lived—and also that a calmer atmosphere was more suitable to the long and careful development of his very original way of thought.

While still at the Ottoschule Grassmann entered for and passed the second theological examination in Stettin in July, 1839. We may note here that he was deeply attached to the study of positive theology throughout his life. After passing his theological examinations he became secretary and then president of the "Pomeranian Central Society for the Evangelization of China." And it is noteworthy in this respect that his last work was on "The Falling Away from Belief."

A few months before he submitted his essay for this last theological test, he was examined by the Berlin Scientific Examination Commission in mathematics and physics. It was in connection with this that an event fraught with great consequences to his lifework happened to Grassmann; for he was set the task, by Professor Conrad of the Joachimsthal Gymnasium, of developing the theory of tides. It is uncertain whether the subject was chosen by Conrad on his own initiative or was suggested by Grassmann himself. In any case it was precisely the practical need which was best calculated to spur him on to the development of his dormant mathematical ideas. Later on he spoke[10] of the necessity, in expounding the claims of a new mathematical discipline, of showing its application. And it seems clear that, faced with the difficulties and complications of Laplace's tidal theory, he was led at once to the idea of transforming analytical mechanics by the introduction of his own rudimentary analytical notions. He found to his delight that the new analysis proved a powerful simplifying tool when applied to the equations of Lagrange's *Mécanique analytique*. This initial success encouraged him to extend his method and to clothe many other conceptions such as exponentials, the angle, and the trigonometrical functions, in the form of that analysis. He was then able to simplify and render symmetrical the intricate formulas of the tidal theory. Furthermore he found that the elimination of arbitrary coordinates so effected left the ideas, their development, and their interrelations much less obscured by analytical machinery.

The thesis Grassmann sent to Berlin in April 1840 was of an unusual size;[11] and, in the opinion of Engel,[12] "judged by the number of new thoughts and methods contained in it, there is only one other to be compared with it—the thesis which Weierstrass submitted a year later to the Commission at Münster." The two works were, however, accorded very different receptions; and it is evident that Professor Conrad had no idea of the remarkable work he had called into being. His report runs: "The test treats the theory of the tides with thoroughness and strength throughout; and he has chosen, not unhappily, a peculiar method which departs in many particulars from the theory of Laplace." It remains an evil omen for the fate of Grassmann's later work that his examination thesis should thus have failed to find recognition. It must be added that Conrad could scarcely have read the work and still less have been able to estimate it at its true value. For he received it on May 26 and returned it five days later at the oral examination—in which Grassmann fared better, being granted full recognition of his mathematical ability.

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[10] In the Preface to the first edition of the *Ausdehnungslehre* of 1844.

[11] It fills 190 pages of royal octavo in the third volume of his *Werke*.

[12] F. Engel, *loc. cit.*

Grassmann probably realized that this thesis on tidal theory was but a first fruit of his methods and that those methods themselves were much more general and capable of immense development. This work he threw himself into with characteristic energy in the next few years. He left the Ottoschule at Michaelmas, 1842, and spent six months teaching at the Stettin Gymnasium; after which he entered the Friedrich-Wilhelm-Schule which had been founded a few years before, and of which his eldest son Justus Grassmann is now the principal.

By 1842 Grassmann had completed the main outlines of his new analytical method. He tried to make the ideas known to his own circle by lectures, in which he showed the power of the new "science of extended magnitudes" by further application to mechanics and crystallography. Desiring to expound his method by reference to well-known results he was led to the barycentric calculus of Möbius and to Poncelet. The first of these illustrations was the "Theorie der Zentralen" (Crelle's *Journal*, Vol. XXIV,

1842) in which, without using his own analysis, he made a general statement in which not only all Poncelet's results but also further important general properties of curves and surfaces are contained as special cases. Such wide generalization is characteristic of his method. In 1844 his *Ausdehnungslehre* was published, being designed as the first part of the complete work. This part, which he proposed to follow up with a second later, he called "*Die lineale Ausdehnungslehre,* a new branch of mathematics."

The fate of this book was a tragic one. It remained unread and unsold until the publisher had to get rid of the whole edition as waste paper. Not even a review was granted to it; and what criticism there was had so little basis of understanding that it led to no deeper study of the work. Gauss wrote of it, in 1844, that its tendencies partly went in the same direction in which he himself for almost half a century had wandered; but there seemed to him to be only a partial and distant resemblance in the tendency. He thought it would be necessary to familiarize oneself with the special terminology to get at the real kernel of the book. Grunert declared that he had not completely succeeded in forming a definite and clear opinion about the work. Möbius, whom Grassmann had asked for a review in some critical journal because he stood nearest to the ideas in the book, answered that this mental relationship only existed in regard to mathematics, not with reference to philosophy; and that he considered himself incapable of estimating and appreciating the philosophical element of the excellent work—which lies at the base of all mathematics. But he added that he recognized that, next to the great simplification of method, the principal gain consisted in the fact that by a more general comprehension of fundamental mathematical operations the difficulties of many analytical concepts are removed.

Without entering in detail into a discussion of the causes of this neglect of Grassmann's work[13] we may note that its great generality, its philosophical form, and its original and technical symbolism were contributing factors which also make it very difficult to give any account of the work for the general reader.[14] But the importance of the ideas hidden away in this forbidding volume may be gathered from the words written of it by Carl Müsebeck many years later: "Earlier than Riemann, Grassmann evolved manifolds of n dimensions in mathematical analysis. In a lighter and less constrained manner Grassmann arrives by his combinatory multiplication at the fundamental principles of determinant-theory, and the elementary solution of various problems of elimination. In him one finds indicated both Bellavitis's Equipollences and Hamilton's Quaternions." And yet the only recognition given by mathematicians to the ideas of Grassmann was the award to him by the Jablonowski Society at Leipsic for a prize essay[15] on the "Geometrical Calculus of Leibniz" in 1846.

It must not be supposed, however, that Grassmann sat quietly down to neglect. He brought out the importance and applicability of his investigation by numerous valuable articles in Crelle's *Journal*, and later in *Mathematische Annalen* and the *Nachrichten* of the Royal Society of Science of Göttingen. Furthermore, in 1845 he published in Grunert's *Archiv*, Vol. VI, a detailed abstract[16] of the *Ausdehnungslehre*, intended for mathematicians. Thirty years later Grassmann spoke to Delbrück[17] with youthful ardor of this period as one of happy restlessness and joy in discovery. Such joy in original work and faith in the power of his mathematical methods he always retained in spite of a succession of disappointments which would have quenched a less ardent spirit.

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[13] See the article below on "The Neglect of the work of H. Grassmann."

[14] An attempt was made to do this by Justus Grassmann in an address delivered at the opening of his school year on April 16, 1909, when the centenary of his father was being celebrated.

[15] *Geometrische Analyse*, published 1847. This treatise is to some extent a substitute for the second part of the *Ausdehnungslehre* of 1844, anticipated in the preface to that work but never written.

[16] Reprinted in the *Werke*, Vol. I, Part I, p. 297.

[17] B. Delbrück, "Hermann Grassmann," Supplement to the *Allgemeine Zeitung*, Oct. 18, 1877.

It is an extraordinary thing that it was not only in his mathematical work that he failed to find recognition, but also in his contributions to physics. In 1845 he published in Poggendorff's *Annalen* a statement of the mutual interaction of two electric stream lines which was re-discovered thirty-one years later by Clausius. In a school syllabus in 1854 Grassmann stated that the vowels of the human voice owe their character to the presence of certain partial tones of the mouth cavity, a view of the nature of vowel sounds which is usually ascribed to Willis and Helmholtz. Of his other purely physical work we may mention his notes on the mixing of colors and his design of a very simple but practical heliostat.[18] Still he continued to hope that the value of his work would be appreciated. He had himself foreseen[19] that the dislike of mathematicians for a philosophical form might deter them from considering his work, and the comments of Möbius and Grunert on this had shown his fears to be well founded. So he yielded to the often expressed wish of Möbius that he should rewrite the *Ausdehnungslehre* in a form more attractive to mathematicians. In the new work, published in 1862, he chose a more deductive method—one moreover which is not altogether suited to the subject matter, but it did succeed in bringing forward more clearly the original operations and characteristics of the *Ausdehnungslehre*. All was in vain. Neither genius nor indomitable energy could contend against so unresponsive an environment.

We must remember that Grassmann's continued output of virile original work was done in the scanty leisure of an energetic schoolmaster. He had been nominated head-teacher at the Friedrich-Wilhelm-Schule in 1847, and five years later he was appointed successor to his father at the gymnasium. There he remained for a quarter of a century. He had hoped that his mathematical writings would win for him some position in which he would have more leisure for research and be in closer contact with other scientific workers. But it must not be supposed for an instant that this lessened his intense interest in the work at hand. He wrote articles on educational subjects as well as a number of text-books for school use. Of these his *Arithmetik*, written in collaboration with his brother Robert showed a strictness in its proofs which made it a good introduction to the theory of numbers. His *Trigonometrie* has a richness of content in small space and an originality of plan not often then found in elementary handbooks.

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[18] A model was constructed by the Stettin Physical Society.

[19] Preface to the first edition of the 1844 *Ausdehnungslehre*.

Müsebeck has questioned some of Grassmann's pupils on his methods of teaching. They appear in the main to agree with Wandel, who says in his "Studies and Characters from Ancient and Modern Pomerania" that he was a lovable and painstaking master whose kindly instruction was sometimes too difficult for them. The lively interest he took in the independence of those he taught is shown by the fact that, according to Schlegel, he formed a society out of every three scholars in his chemistry class, the members of which had to demonstrate and lecture to the others on some substance and its combinations. The pleasant footing he established between himself and his classes may be judged from the fact that they were willing to co-operate in classwork with him when in later years he had to be taken to school in a wheeled chair. Whenever any of his old pupils speak of him they do so with the greatest admiration and respect.

It is difficult in thus giving an account of Grassmann's educational and scientific activity to avoid at the same time conveying the impression of a mere enthusiastic pedant. It does not seem that there could have been time for anything else. And yet such a view would be widely removed from the truth. For in the midst of all these exacting duties he had many social and general interests. In 1848 he took an active part in politics, expressing anti-revolutionary sympathies; he attempted to introduce a German plant-terminology into botany; and his early developed love for music found expression in organizing an orchestra of his scholars and in collecting numerous folk-songs, which he set for three voices, to be sung in his family.

We have been led, by the necessity of obtaining some idea of the actual conditions under which Grassmann worked, to speak of his later life. We must now return to the time when he first began to realize how slight a recognition was to be accorded to his mathematical writings—that is to say about the year 1852. Great as was his inner sureness of the value of the work, yet his was not the type of mind to be satisfied with a partial success. And so he took the astonishing (and almost unprecedented)[20] step of turning his attention to another field of knowledge altogether and quickly winning the recognition of experts. The pliability of his genius enabled him to force his way into a new subject, philology, and to produce results of outstanding merit in it.

B. Delbrück[21] gives an interesting account of how Grassmann turned to philology. The rules of the traditional school grammar with its mass of exceptions must have been painful to his mathematical understanding, and so he first planned a grammar and reading book in which scientific laws replaced the old rule-of-thumb methods wherever possible. It is natural therefore that he should next turn his attention to that sphere of language in which such laws are most easily recognizable, namely phonetics. His first attempt in the realm of comparative philology was on this subject. It was an article, published in 1859, on the influence of *v* and *j* on neighboring consonants, and on certain phenomena in connection with aspiration. Delbrück expresses the opinion that his work in this field is not distinguished either for breadth of scholarship, since he worked with few books, or for etymological depth. "But," he says, "it is the clearness of reflection which penetrates into all corners of the subject, the persistence with which the material has been so long accumulated until it became possible to reach the simplest formulation of the governing law, and the untiring nature of the mathematical abstraction which in these undertakings so clearly comes to light."

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[20] The equally neglected English genius Thomas Young combined mathematical and philological ability.

[21] B. Delbrück, *loc. cit.*

Grassmann must have quickly recognized how valuable in all researches into comparative philology a deep acquaintance with the oldest Indian languages would be, and he determined with his usual persistency to make himself at home in the hymns of the Vedas. These Sanskrit studies led him to the production of works which rendered his name famous. In 1861 he had only the first volume of the upright text and scarcely half of the Böhtlingk-Roth dictionary. Yet with these means he succeeded in mastering the extraordinary difficulties of the texts, and began his dictionary and translation of the Rig-Veda. He arranged his dictionary in an original manner so as to be able to give the meaning of each form according to the place in which it occurred. Although Delbrück credits the first volume of the dictionary with etymological value for its grammatical subdivision of the roots, yet he regards the arrangement just mentioned as unphilological. It aims less at giving definite historical and philological information than at making successive attempts at explanation. As, however, the work progressed, aided by the stream of material reaching the author from the growing Roth dictionary and elsewhere, it became more philological. Still the method pursued was the same, and Grassmann completed the translation side by side with the dictionary. For long these works formed a useful tool in attacking the difficulties of the Vedas. The recognition of experts was worthily expressed by Rudolph Roth, on whose word the University of Tübingen conferred upon Grassmann the honorary degree of Doctor of Philosophy. He spoke of him as a man *qui acutissima vedicorum carminum interpretatione nomen suum reddidit illustrissimum.*

During this period of his life when he was winning fame in another sphere of work, Grassmann's mathematical writings were gradually obtaining the recognition which was their due. Toward the end of the sixties considerable attention was paid by mathematicians to higher algebra, and the quickening of thought along those lines made recognition much more likely. Hermann Hankel in his *Theorie der complexen Zahlensysteme* of 1867 was the first to call attention to Grassmann's work. Clebsch[22] also shortly afterward accorded him a full measure of admiration. Grassmann[23] believed that Clebsch would have fertilized the theory of extension with far-reaching new ideas of his own if death had not cut short his promising career.

Some of the younger teachers at the Stettin Gymnasium had become pupils of Grassmann; and one of these, the mathematician Victor Schlegel, in his *System der*

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[22] Clebsch, *Zum Gedächtniss an Julius Plücker*, 1872.

[23] See preface to second edition of the *Ausdehnungslehre* of 1844, published in 1878.

Raumlehre (1st part 1872, 2d part 1875) made his works more accessible by a clear exposition and application of them. The best kind of approval from authorities came to him in their use of his methods in various fields; and Grassmann himself, after a long interval, again took up his mathematical labors. Of the many articles from his pen, we may mention especially that on the application of his work to mechanics,[24] because it was in this domain that he considered the theory of extension to be particularly successful. He expressed the desire that it might be granted to him to write a treatise on mechanics based on his principles. This was denied him. He lived, however, to see a second edition of his ill-fated *Ausdehnungslehre* of 1844 called for; and died, while it was passing through the press, on September 26, 1877, in his sixty-ninth year. To the last, in spite of great bodily suffering, he retained his vigor and enthusiasm. Five essays published in the year of his death testify to this.

It is a pleasant thing to think that he received such rich recognition before he died; though it must always remain a source of regret that he never succeeded in obtaining the position he hoped for, which would have enabled his powers to be more fully developed and his influence more widely expressed. And yet, there can be no cause for sorrow if we think of the fortitude of this strong soul, and remember the firm conviction expressed in the closing words of the introduction to the *Ausdehnungslehre* of 1862, that his mathematical ideas would some day arise again, though perhaps in a new form, and become part of living thought. To some extent that conviction has proved a justifiable one. The publication of his Collected Works was suggested by Professor Klein of Göttingen. After obtaining the consent of Grassmann's relatives he laid the matter before the Royal Saxon Academy of Sciences in October, 1892. A committee was formed and F. Engel made chief editor. The first part of the first volume appeared in 1894.

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[24] "Die Mechanik nach den Principien der *Ausdehnungslehre*," *Math. Annalen*, Vol. XII, 1877.

Since then there have been many works on the calculus of extension, but it can scarcely be held that they have done more than make a beginning of the development of the suggestions in Grassmann's work. What has been done has been mainly in the domains of spatial theory and higher algebra; mechanics remains still burdened with traditional coordinate systems. This is the more remarkable since the principle of relativity, with its demand for a generalized dynamics of which ordinary dynamics is a special case, offers such a promising field of application.

There is usually, in the sphere of thought, a rational explanation of apparently irrational facts. A minute influence translated into action by the mass of thinking men may give rise to the spirit of their age; and thus its effects, and the negative effects may be just as great as the positive, carried forward in ever-increasing circles to distant generations. So it has been with whatever lies at the base of the neglect of Hermann Grassmann. There has been bequeathed to us something like an unreasoning distaste for his and similar analytical methods, from which has arisen the need for a definite effort to break the spell of the past. The formation of an "International Association for Promoting the Study of Quaternions and Allied Systems of Mathematics" took its origin from such a need.[25] It may therefore be that a just estimate both of the value and limitations of Grassmann's work will only come by the application of a critical method of wider scope than those of his own period. Indications are indeed not wanting that in the modern theory of transformation-groups[26] lies the criterion for a final judgment.

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[25] P. Molenbrock and Shunkichi Kimura, letter to *Nature*, Oct. 3, 1895.

A. E. HEATH.[27]

BEDALES, PETERSFIELD, ENGLAND.

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[26] Lie and Engel, *Theorie der Transformationsgruppen,* Vol. II, p. 748; M. Abraham and P. Langevin, "Notions géométriques fondamentales," *Encyc. des sciences mathématiques,* Tome IV, Vol. 5, p. 2.

[27] I wish to thank Miss Vinvela Cummin and Mr. R. E. Roper for help in the translation of materials for this sketch.

THE NEGLECT OF THE WORK OF H. GRASSMANN.

IT must not be supposed that the neglect of Hermann Grassmann's mathematical work by his contemporaries is merely an incident of his biography. Its consideration involves a much larger question, because Grassmann's fate was shared by other mathematicians of the period in whose work stress was laid on form rather than content. The distinction between the two may be illustrated by reference to the mathematical treatment of quantity. As soon as analysis had generalized that idea so as to include complex quantities, a mathematics based on formal definitions and of a general character could be developed to include them. The meaning of the propositions of such a calculus need not enter into this study. The propositions would constitute a formal deductive series which could be developed without any reference to content. That Grassmann was a pioneer in the movement which made magnitude subordinate and posterior to a science of form was recognized by Hankel,[1] who says, "It was Grassmann who took up this idea for the first time in a truly philosophical spirit and treated it from a comprehensive point of view." In the Introduction (A) to the *Ausdehnungslehre* of 1844 Grassmann puts the matter thus: "The chief division of all sciences is that into real and formal. The former sciences image in thought the existent as independent of thinking, and their truth consists in the agreement of the thought with the existent; the latter sciences on the contrary have for their subject-matter that which has been determined by thought itself, and their truth is shown in the mutual agreement between processes of thought." He goes on to consider mathematics and formal logic as branches of a general science of form, and seeks to dissociate this science from such real sciences as the geometry of actual space, although it must form the basis on which all such are built.

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[1] *Theorie der complexen Zahlensysteme,* p. 16.

That the neglect accorded to Grassmann had nothing to do with any accident of birth or position is shown by the fact that Leibniz, whose name was famous in both mathematical and philosophical circles, shared the same fate in regard to his *Dissertatio de Arte Combinatoria* and later writings of the same kind, in which he sought to set up a formal symbolical calculus with similar aims. Of Grassmann's contemporaries who worked in the same field, we need mention only George Boole (1815-1864) who failed to obtain anything like a due recognition of his genius; and Sir. W. R. Hamilton whose early papers on quaternions were regarded as mere curiosities. Even when the applications of these generalized formal methods to the founding of a calculus of directed quantities of immediate value to physics had been made, we find the important work of Willard Gibbs waiting for years before it became known and made full use of. If, then, we are to explain the neglect of Grassmann's work we shall have to analyze the causes of the apathy and mistrust with which all such work has been received.

The view held by Carl Müsebeck is that in the almost exclusively philosophical form of representation, *which however was grounded in the whole system,* we have to seek the reason why the contemporaries of Grassmann drew back in terror from deeper study of his early work. He says[2]: "Such a height of mathematical abstraction in which, with the help of a new calculus, laws are inferred in abstract regions about the mutual dependence of abstract constructions in which not even the character of the spatial is maintained, although at the conclusion of almost every section it is shown how the new method could be used with advantage, was never before known." That this has been a very important factor cannot be doubted. Dislike of the philosophical form of his work was expressed to Grassmann by the few mathematicians who noticed his first *Ausdehnungslehre*. He himself says in the preface to the second edition of this book that he expected the work to find fullest recognition from the more philosophically inclined reader. It is only necessary to refer to the application and extension of his ideas which have come from A. N. Whitehead[3] in England and from G. Peano[4] and C. Burali-Forti[5] in Italy to show how well-founded this forecast was. But the analysis cannot rest there. We must inquire further how this dislike arose.

J. T. Merz[6] in his chapter on "The Development of Mathematical Thought in the 19th Century," inclines to the view that a definite distaste for a philosophical form had set in among German mathematicians as a part of the reaction against the exaggerations of the metaphysical unification of knowledge in the schools of Schelling and Hegel. But mathematicians in modern times have, on the whole, been singularly unaffected by philosophical movements. Furthermore the calculus of extension and allied systems have not fully come into their own even in our own day, when wide syntheses are eagerly sought. It seems to the present writer that it is in the attitude of the plain anti-metaphysical mathematician that we must seek for the explanation of the want of understanding which leads to mistrust of philosophical form. An immense amount of prejudice barred the way to the full development of a general science of form—prejudice due to non-realization of the purely formal claims of such a calculus.[7] And if we could get at the bottom of this not altogether unreasoning mistrust it might be possible to clear away some of the hindrances to a proper understanding of the fundamental importance of Grassmann's work.

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[2] In his memoir of Hermann Grassmann, Stettin, 1877.

[3] *Universal Algebra*, Cambridge, 1898.

[4] *Calcolo Geometrico secondo l'Ausdehnungslehre di H. Grassmann*, Turin, 1888.

[5] *Introduction à la géométrie différentielle, suivant la méthode de H. Grassmann*, Paris, 1897.

[6] *History of European Thought in the 19th Century*, Vol. I, p. 243.

To do this we must push our analysis a step further. What steady cause can have been operating over such a long period which could so affect the attitude of the individual as to create what amounts almost to a general blindness to the importance of a whole body of contributions to thought? I believe that the root of the matter lies in wrong principles of instruction. It may be that this at first sight appears too small an influence to have such consequences; but so did the minute geological influences of the uniformitarians to those who sought for explanations in more dramatic cataclysms. It is as unscientific to neglect the unobtrusive but persistent influences of educational methods on pure thought as it would be to treat of the social conditions of a people without taking into account their mind-development.

We will only give one well-recognized example of the importance of methods of exposition on mathematical history. Merz places Gauss at the head of the critical movement which began the nineteenth century. He adds,[8] however, that it was not to him primarily that the great change

[7] Cf. the article below on "The Geometrical Analysis of Grassmann and its Connection with Leibniz's Characteristic," § 2.

[8] *Op. cit.*, Vol. II, p. 636.

which came over mathematics was due, but to Cauchy. Gauss, while issuing finished and perfect though sometimes irritatingly unintelligible tracts, hated lecturing; in contrast to this Cauchy gained the merit, through his enthusiasm and patience as a teacher, of creating a new school of thought—and earned the gratitude of the greatest intellects, such as Abel, for having pointed out the right road of progress. But it is not so much upon the manner of exposition of original mathematicians themselves that stress must be laid. It has without doubt often happened that writers of great analytical insight have failed to see that it is no more a descent to a common level to seek out and use the best methods of enforcing consideration of their work, than it is to use a printing-press instead of a town crier the more effectively to reach their audience. Grassmann himself, however, did all that was humanly possible in this way, although Jahnke is of the opinion that he was inclined to the belief that even first instruction should be rigorous; and kept back applications until too late. It is rather that teaching methods in general during the nineteenth century have always lagged too far behind discovery. And so they have left the students of one generation, who are the potential original workers of the next, with minds unreceptive to newer and more delicate methods. It might be urged that this would affect equally all branches of mathematics, but I think it can be shown that it is on the reception of such fundamental analytical methods as Grassmann's that its evil influence more particularly falls.

It is quite obvious that the subject must be limited if we are to deal in detail with the suggested effects of inadequate educational methods. So I shall confine myself in what follows to the consideration of the difficulties which beset the path of the teacher who has to explain the ordinary concepts of mechanics; and attempt to show how failure to realize the nature of those difficulties tends to produce an unreceptive attitude to modern analysis. I have chosen this subject for two reasons. Firstly, it seems to me that if the concepts of mechanics were properly treated they would finally appear to the pupil as useful constructions instead of as the dogmatically asserted existents they are still commonly held to be; and so the formal science underlying the real science of mechanics would naturally arise for him as the final result of analysis, and not as the unreal fabric of a philosopher's dream. And secondly, it is the domain to which the various "extensive algebras" have peculiar applicability, as Grassmann himself felt strongly. It is highly significant therefore that it is precisely Grassmann's suggestive applications to mechanics whose neglect is the most noticeable. That this is so is. on my view, because sounder and more philosophical notions of geometrical as opposed to mechanical concepts were already coming into exchange in Grassmann's own day so that geometrical applications were thereby rendered more understandable.

At the very outset of our discussion we are faced with the difficulty that so much difference of opinion exists between teachers of mechanics that many have been forced into the conclusion that, since the enthusiast with an unphilosophical method of his own can yet reap good results, method is unimportant. This, of course, is only partially true. If it were wholly true it would mean an end to all possibility of coordination—an end, in fact, to the claims of education to be a science. To grant that education is an art is not to forego all its claims to be a science. For we must regard all art as applied science "unless we are willing, with the multitude, to consider art as guessing and aiming well."[9] Beneath the apparent chaos of opinion on the teaching of mechanics there is however some order if one can avoid certain sources of confusion which have led to superficial differences of opinion where nothing deeper exists.

One source of confusion is the absence of a clear idea of the difference in educational theory between an impersonal *principle* and the more personal element—the *method* of applying the principle. This distinction is insisted on by Mr. E. G. A. Holmes,[10] and seems a real one. If once we realize it we can see how it is possible for there to be fairly well accepted scientific principles of teaching at the same time as a wide divergence of method in use by different teachers under differing conditions. And indeed if one looks carefully into much of the polemical writing on mechanics teaching it is seen to be caused less by fundamental differences of principle than by differences of method. It is still more necessary to clear away a second source of unsatisfactory discussion. A superficial glance through the mass of controversial writing on science teaching in recent years would lead one to suppose there was a sharp division of principle between those who believe in a logically ordered course with emphasis on what one may call the instructional method, and those who prefer a looser, more empirical, treatment usually embodying heuristic methods. It would be possible, however, to reconcile many of the combatants if they could be persuaded to see that so direct an opposition is far too simple a statement of the problem, and that each may be partial statements of the real solution. And this becomes possible, I think, if once the disputants grant the importance of the biogenetic or embryonic principle as applied to education—the principle, that is to say, that the development of the individual is a recapitulation of the development of the race. It seems strange that it should be necessary at this stage to call attention to a principle so well known[11] and so much applied, and yet one often has the spectacle of a successful teacher of higher classes urging the claims of logical order against an equally successful empiricist whose experience has been with younger pupils. The truth is, of course, that no one method is applicable to all ages. If the biogenetic law holds, then the natural principle would be to use, in general, modes of teaching a subject similar at each stage to those by which the race has gathered its knowledge of that subject. In mechanics this would mean that a more rigidly logical course would follow empirical experiments and the handling of simple machines.

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[9] Reference to Plato, *Philebus*: G. Boole, *The Mathematical Analysis of Logic*, note p. 7.

[10] E. G. A. Holmes, *The Montessori System of Education*, English Board of Education Pamphlet, No. 24, p. 3.

We will now pass on to our main investigation of the factors which must be taken into account in avoiding the creation of an atmosphere uncongenial to a final abstract analysis. In doing so I will indicate what appear to be the general principles by which one must work in giving to beginners living ideas of the entities of mechanics, and failure to comply with which leads to the production of passively instructed, rather than of irritable and responsive, organisms. The concepts of mechanics are produced from the raw material of experience by the process of abstraction, and a beginner must therefore pass through an experimental stage before he is introduced to the logically defined concepts themselves. In fact he must first use and handle rough ideas and thence be led to build up the more rigidly exact definitions of them for himself. It follows from this that any information we can glean about the actual historical process by which man came to form and use concepts may be of vital importance to a teacher. In mechanics particularly, where the concepts are less obvious than in geometry (the first ideas of force, mass, acceleration and energy, regarded however not as constructions but as real entities, were only developed to any clearness after Galileo—that is at quite a late stage in man's history) any fogginess about their nature and use means endless confusion; and that accounts for most of the difficulties commonly experienced.

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[11] It is a very remarkable thing that De Morgan in his *Study and Difficulties of Mathematics*, first published in 1831, or 28 years before the *Origin of Species*, should have stated this principle so concisely in the words (p. 186) referring to discussions of first principles: "the progress of nations has exhibited throughout a strong resemblance to that of individuals."

It was Locke who first plainly showed how concepts arise from the material of immediate perception. If we think of the flux and confusion of our perceptions—the colors, sounds, smells, sensations of touch, at any instant we find our attention drawn to some more insistent parts of that flux. When these continually recur we use nouns, adjectives and verbs to identify them. Such is the beginning of the formation of concepts. These are regrouped to form other concepts. Thus a wide experience of animals would lead us to group them and to speak, for example, of a class "dog." Once classed we can treat all instances as having the general properties of the class. The practical advantages are obvious. "The intellectual life of man consists almost wholly in his substitution of a conceptual order for the perceptual order in which his experience originally comes," says William James.[12] Once concepts are formed they enable us to handle our immediate experience with greater ease. And by building up more and more complex concepts and tracing the connections between them we create our mathematics and our sciences.

Even animals may form rough concepts.[13] A dog by experience comes to know the difference between "man" and other animals. Furthermore if he met a dummy man he would soon find out that the reactions he ordinarily associated with "man" failed to be reproduced, and so would reject that experience for his man-class. In a similar way man must have formed concepts becoming more and more complicated but more firm in outline as his experience became richer. But it is to be noticed that the growth of concepts in a body of experience depends on the number and interest of our observations in the region concerned. For this reason interest in, and consequent familiarization with, simple machines and mechanical toys may well be the child's best introduction to mechanics. Model monoplanes, an old petrol engine from a motor cycle, pumps, a screw, levers, a jack, Hero's turbine model—all these can easily be got at; few young children will show no interest, while many of them will possess already in these days of mechanical toys a considerable knowledge of manipulation. Simple explanations of the working of such apparatus are absorbed with astonishing readiness. In larger schools where there is an engineering workshop this method of introducing young boys to mechanics by way of machinery has been tried with considerable success. Knowledge gets picked up as it were "by contact." The concepts which arise at this stage are necessarily crude—general ideas of force, speed, work and friction; this latter is, of course, one of the first things to notice—not the last to be dealt with as is usually the case. Simple as these considerations are, they are not yet fully appreciated. The London Mathematical Association's *Report on the Teaching of Elementary Mechanics* suggested some time ago that the phrase "Mechanical Advantage" be replaced by "Force-Ratio." For beginners neither of these is intelligible; but they very soon know "how much stronger" a machine makes you. And that conception is quite good enough for them to use.

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[12] *Some Problems of Philosophy*, p. 51.

[13] This treatment of the origination of concepts is founded largely on that of E. Mach in his chapter on Concepts in the volume *Erkenntnis und Irrtum*.

In introducing simple mechanical concepts to beginners, therefore, the principle to use is that the concepts must arise naturally from experience and not be handed out as definitions. Dictated definitions not founded on sufficient knowledge of facts are flimsy constructions ready to fall at the first breath of difficulty. They do not perform that primary function of concepts of helping one to classify and handle facts, because the facts to be handled are not in the mind when the concept is formulated. "How much stronger a machine makes you" is a phrase which reminds the hearer at once of the assistance it gives him in grouping machines and using them intelligently for different purposes. A note-book definition of "mechanical advantage" is likely to present another arithmetical puzzle instead of serving to remind the learner of the solution of old ones. The principle here advocated was well expressed in the discussion on mechanics teaching at the British Association in 1905 by the president of the section, Professor Forsyth. He said, "What you want to do in the first instance is to accustom the boys to the ordinary relations of bodies and of their properties, and afterward you can attempt to give some definitions which will be more or less accurate; but do not begin with the definitions, begin with the things themselves." And the philosophical basis for the principle is, that the significance of concepts is always learned from their relations to perceptual particulars, their utility depending on the power they give us of coordinating perceptual facts. From this it follows further that concepts and names should never be introduced where there is no direct and immediate gain in so doing. Such terms as "centrifugal" and "centripetal" forces, and the endless discussion to which they lead, are thus beside the mark. "Force toward, or away from, the center" does all that is necessary without introducing new words of really less precision.

It should be noted that some of the crude concepts arrived at in the early stages are really, when one comes to analyze them, very complex, and Ostwald's warning[14] against the error of supposing that the less simple concepts have always been reached by compounding simple ones has application here. As he says, complex concepts often in origin have existed first. We can now see more clearly why the teacher of mechanics so often complains of the difficulty of giving the average child a satisfactory notion of force.[15] The difficulty is largely due to the teacher who knows the concept to be complicated, and seeks to define it in terms of mass-acceleration—thus involving two more concepts, one of which (mass) is at least as difficult to understand as force. A rough idea of force, considered simply as a "push" or a "pull," can be assimilated at a very early stage; that of mass-acceleration must come very much later.

The bearing of this preliminary stage in the formation of concepts on our main thesis may now be traced. It is quite evident that the individual has very limited powers of absorbing the logically ordered account of a science in which stress is laid on abstract notions before such notions have grown up naturally by use. Now this difficult step for the beginner from the perceptual to the conceptual is very similar to that which leads from ordinary mechanics to such a treatment of the subject as that of Grassmann. Both lead into regions of greater abstraction. In the latter case we can get rid of concepts in so far as they relate to the existent, and reach a statement of mechanical principles in terms of a generalized form-theory. We may illustrate, roughly, the meaning of this by the following analogue. At different stages in the history of physics various theories of light have been held. The concepts used in these theories (corpuscle, elastic-solid ether, electro-magnetic medium) have possessed widely different "qualities"; but the equations expressing the relation between the conceptual elements have throughout possessed similarity of form. A science of form would hence lay emphasis on the invariant relations, refine away the particular concepts, and leave a much more abstract and generalized science.

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[14] Ostwald, *Natural Philosophy*, p. 20.

[15] Cf. C. Godfrey, Brit. Association *Report on Mechnics Teaching*, p. 41.

But if racial development is in the main similar to the progress of the individual this will explain the great difficulty experienced by whole generations of mathematicians in understanding work of the type of Grassmann's.

Furthermore, it is at this point that defective scientific training looms into importance. For unless great care has been taken in avoiding the too early definition of concepts, a rigid view of them is promulgated. The older dogmatic and orderly methods of teaching tended inevitably to this. The consequence was that when the time came for polishing and development of the concepts obtained, and for the deliberate building up of more complex ones—it was found that the capacity for subtle generalized views had been destroyed. A mind forced into passivity and filled with inert knowledge cannot suddenly be brought to discard it in response to the stimulus of a tentative generalization. To take a simple example, the idea of a new kind of addition, applicable to vectors, shocks and confuses a pupil who has been dogmatically instructed in algebra as though it were a sacred rite. As with the child under such a system, so with the generation of which he forms a part. Jahnke states that many mathematicians were put off by meeting in Grassmann's work a product which equals zero without either factor doing so. Formal logical development often leads to conclusions which are not capable of any mental image.[16] Such abstractions are out of reach of those who have never been freed from the confines of the existent world.

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[16] Cf. F. Klein, *The Evanston Colloquium*, Lecture 6.

Cajori[17] in a notice in 1874 of the publication called *The Analyst,* Des Moines, Iowa, said that it bore evidence of an approaching departure from antiquated views and methods, of a tendency among teachers to look into the history and philosophy of mathematics. My thesis is that such a movement, which certainly has not yet been realized, would remove the main cause of the neglect of Hermann Grassmann's work, which even in these days is often granted the kind of recognition accorded to certain literary classics, which are famous but never read. Perhaps it is an earnest of the future that the copy of *The Analyst* referred to by Cajori contained a brief account[18] of the essential features of Grassmann's *Ausdehnungslehre.*

A. E. HEATH.

BEDALES, PETERSFIELD, ENGLAND.

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[17] *Teaching and History of Mathematics in the United States.*

[18] Translated by W. W. Beman of the University of Michigan.

THE GEOMETRICAL ANALYSIS OF GRASSMANN AND ITS CONNECTION WITH LEIBNIZ'S CHARACTERISTIC.

§ I.

BY a curious turn of fate Grassmann wrote, in the introduction to his "Geometrical Analysis," concerning Leibniz's early work on the same subject, words which were to apply with prophetic force to his own *Ausdehnungslehre.* "When the special power of a genius....is so revealed that he is able to grasp and extend the ideas toward which the development of his time is directed, and so appears representative of his period, then that power shows itself still more remarkable when it can seize ideas in those realms of thought in advance of their day and forecast for hundreds of years the line of their development. While ideas of the first kind are often developed simultaneously by the outstanding spirits of the age, as when both Leibniz and Newton founded the differential calculus—a certain stage of fruition being reached—ideas of the latter kind appeared as the special characteristic of the individual, the innermost revelations of his mind into which only a few elect contemporaries could enter and have a foreshadowing of the developments which were to spread from them in the future. While the first received great applause and aroused movement in their own day, because they represent the summit of the epoch, the others for the most part fall without effect in the contemporary period since they are only understood by a few, and then only partially. Often afterward does such thought become the seed of a rich harvest. That this great idea of Leibniz—namely, the idea of a true geometrical analysis—belongs to this preparatory and, as it were, prophetic class cannot be doubted for a moment. It has also shared the fate of such. Indeed by a special ill favor of circumstances it has remained hidden far beyond the time when it might have had a powerful influence. For even before it was brought out of its hiding place by Uylenbroek, paths toward a similar analysis had been made in other ways."

At the time when these words were written Grassmann could have had no idea of the disappointment which was to come to him in the neglect of his own work. The first edition of the *Ausdehnungslehre,* or theory of extended magnitudes, had been published in 1844 and had received no attention from mathematicians with the exception of a few individuals. Grassmann, however, believed that recognition was only a matter of time and sought to bring out the importance and applicability of the new analysis. For the year 1845 (but extended to 1846 to coincide with the two hundredth anniversary of Leibniz's birth) the Jablonowski Society of Leipsic set a prize essay demanding the restatement or further development of the geometrical calculus discovered by Leibniz or the setting up of a similar calculus; and the award was made to Grassmann for the essay, printed in 1847, from which I made the above quotation. This was the first and the only acknowledgment of the value of his work which he received from mathematicians until long after many of the ideas he formulated had been reached and applied by other methods and other thinkers.

I have laid stress on the similarity of treatment meted out to the fundamentally important work of the two men because I believe that in some elements of its explanation lies the clue to unravel the difficulties of their subject matter and connection with each other. The more general aims of both Leibniz and Grassmann were the same—the setting up of a convenient calculus or art of manipulating signs by fixed rules, and of deducing therefrom true propositions for the things represented by the signs, for use as a generalized mathematics. In each case their geometrical calculus was a particular application to geometry of a wider calculus for which each desired more than mere applicability to mathematics.

In a letter to Arnauld, dated January 14, 1688, Leibniz writes[1]: "Some day, if I find leisure, I hope to write out my meditations upon the general characteristic or method of universal calculus, which should be of service in the other sciences as well as in mathematics. I have definitions, axioms, and very remarkable theorems and problems in regard to coincidence, determination, similitude, relation in general, power or cause, and substance, and everywhere I advance with symbols in a precise and strict manner as in algebra. I have made some applications of it in jurisprudence." Similarly Grassmann[2] says: "By a general science of symbols (*Formenlehre*) we understand that body of truths which apply alike to every branch of mathematics, and which presuppose only the universal concepts of similarity and difference, connection and disjunction." The symbols are made so general as to be applicable to both logic[3] and mathematics, although in the *Ausdeh-*

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[1] George R. Montgomery (trans.), *Leibniz: Discourse on Metaphysics, Correspondence with Arnauld, and Monadology*, p. 241.

[2] *Ausdehnungslehre* of 1844, p. 2.

[3] The application of such a general science of symbols to formal logic was made by both H. Grassmann and his brother Robert.

nungslehre they are only applied to the domain of mathematics.[4]

It is clear that both Leibniz and Grassmann, but especially the former, claimed great scope for their calculus, a fact which tended to make their writings generalized and difficult to understand. In the preface to his *Universal Algebra* (1898) Professor Whitehead expresses his belief that lack of unity in presentation (which of course would be the tendency in dealing with a method applicable to many fields) discourages attention to such a subject. But that is not all. A new mathematical method, to make itself known, has to appeal in the main to mathematicians and not to philosophers. So that a wide and philosophical treatment is apt to be discounted by the ordinary man who thinks logic can be made to prove anything.

§ 2.

Before we condemn this attitude we must first of all inquire as to what exactly the common man means by the dangers of logic. What he really fears is not logic but fallacy. Without realizing it he distrusts a mechanical dexterity in reasoning because the attainment of truth depends not only on a facility in manipulating logical processes but also on the sifting of first principles. When Leibniz claims for his *characteristica universalis* or "universal mathematics," the germ of which he produced in his *De arte combinatoria* published when he was twenty, that ". . . . there would be no more need of disputation between two philosophers than between two accountants. For it would suffice to take their pencils in their hands, to sit down to their slates, and to say to each other (with a friend as witness, if they liked): Let us calculate"—he is running counter to the plain man's knowledge that there are two parts of a logical process, the first the choosing of an assumption, the second the arguing upon it.

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[4] In the *Ausdehnungslehre,* however, are expressions directly applicable to logic, e. g., there is the generalized expression for the result of division $C + O/B$ where O/B is an indefinite form (p. 213)—an anticipation of Boole's use of O/O to symbolize perfect indefiniteness, as pointed out by Venn in his *Symbolic Logic*, note p. 268 (2d ed.).

Now Leibniz realized of course that premises are required first, but he thought they could be obtained very simply. By analyzing any notion until it was simple he thought that all axioms or assumptions followed as identical propositions. Thus he was led, by his view of ideas, to believe that even the axioms of Euclid could be proved. So in his *New Essays*, "I would have people seek even the demonstration of the axioms of Euclid.... And when I am asked the means of knowing and examining innate principles, I reply....we must try to reduce them to first principles, i. e., to axioms which are identical, or immediate by means of definitions which are nothing but a distinct exposition of ideas." This is connected with his view that all our ideas are composed of a very small number of simple ideas, which together form an alphabet of human thoughts. But, as Couturat remarks,[5] there are many more simple ideas than Leibniz believed; and furthermore there is no great *philosophical* interest in such. "An idea which can be defined or a proposition which can be proved, is only of subordinate philosophical interest."[6] It is precisely the business of philosophy to deal with the primitive, intuitive assumptions on which any calculus must be based.

So the plain man is to some extent justified in his mistrust of the uncritical application of a calculus.

§ 3.

It is very necessary, however, to see exactly what is, and what is not, here granted to the plain man. It is true that in using a calculus we must be careful not to over-emphasize the results at the risk of forgetting the premises from which they have been obtained. But that being admitted, thus making the final development of the universal characteristic a matter not of philosophy but of a sort of generalized mathematics of which formal logic[7] and geometry are special cases, it does not follow that there must be limits to the applicability of the calculus in these spheres. Yet that is what the modern representative of our plain man asserts. His criticism of a logical calculus has put on a more philosophical form, but remains essentially the same. Henri Poincaré may justly, I think, be taken as such a representative. For he says, "I appeal only to unprejudiced people of common sense....they [the logisticians] have shown that mathematics is entirely reducible to logic, and that intuition plays no part in it whatever."[8] This belief led Poincaré to the view that, since he knew from his own experience as a mathematician of great insight the important part intuition plays in mathematical discovery, therefore the *nature* of mathematics cannot be logical.

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[5] L. Couturat, *La logique de Leibniz*, p. 431.

[6] B. Russell, *The Philosophy of Leibniz*, p. 431.

This reasoning is founded on a very common fallacy which I will call the genetic error—the error, namely, which lies in the assumption that the origin of a thing in some way determines its nature.[9] If this assumption is made it follows that since intuition plays a part in discovery, the nature of mathematics cannot be purely formal, and therefore it cannot be expressed in terms of symbolic logic. Now all such references to the origins of mathematics are irrelevant. Once the premises have been made, and that is where intuition comes in, symbolic logic is merely "an instrument for economizing the exertion of intelligence."[10] The mind, being relieved of unnecessary work by a good symbolism, is set free to attack more difficult problems; for as Professor Whitehead says,[11] "Operations of thought are like cavalry charges in a battle—they are strictly limited in number." Nor is that the only advantage of this modern development of Leibniz's universal mathematics. It "has the effect of enlarging our abstract imagination and providing an infinite number of possible hypotheses to be applied in the analysis of any complex fact."[12] And so it lends itself to the production of just such novel fundamental hypotheses as are needed in subjects like the dynamics of relativity.

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[7] Leibniz himself foresaw this development carried out by Boole, Peano, Frege, Whitehead and Russell and their school of symbolic logicians. In fact he made discoveries in this field but did not publish them because they contradicted certain points in the traditional doctrine of the syllogism. In some points he even advanced beyond Boole (See Couturat, *op. cit.*, p. 386).

[8] *Science et Méthode*, p. 155; cf. also C. J. Keyser in *Bull. Amer. Math. Soc.*, Jan. 1907, pp. 197, 198.

[9] This error has been very common in philosophy. It underlies much argument against rationalism, denying that knowledge reached empirically can be anything other than empirical. (Cf. Leibniz, *New Essays*, IV, 1 § 9, against Locke.) It is at the basis of many criticisms leveled against any generalization of number, since the idea of number arose from perceptual experience. It vitiates pragmatism, which inquires into the causes of our judging things to be true in order to get at the nature of truth. (See B. Russell, *Philosophical Essays*, p. 110.)

So finally, we must say of the symbols of a universal calculus what Hobbes said of words, "They are wise men's counters, they do but reckon by them; but they are the money of fools." Yet it must be recognized that when it is confined to dealing with mathematics in its widest sense (taken to include formal logic),—within the limits imposed on his own calculus by Grassmann, in fact,—it serves as a powerful and legitimate tool.

§ 4.

This discussion of the neglect and mistrust of mathematicians for the generalized calculus of both Leibniz and Grassmann has, I hope, shown what the nature of such a calculus is. Moreover, it accounts for the long period which elapsed before their fruitful application of these methods of calculation to special fields obtained the notice they deserved.

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[10] W. E. Johnson in *Mind,* N. S., Vol. I, pp. 3, 5. Cf. Stout, "Thought and Language," *Mind,* April, 1891.

[11] *An Introduction to Mathematics,* Home Univ. Library, p. 59. See also P. E. B. Jourdain in *The Monist,* Jan. 1914, p. 141.

[12] B. Russell, *Our Knowledge of the External World,* pp. 58, 242.

The particular application we are here concerned with is that to geometry. In a letter to Huygens of September 8, 1679, Leibniz complained that he was not satisfied with the algebraic methods, and adds: "I believe that we must have still another properly linear geometrical analysis, which directly expresses *situm* as algebra expresses *magnitudinem*. And I believe I have the means for it, and that one could represent figures and even machines and movements in symbols, as algebra represents number or magnitude; I am sending you an essay which seems to me notable." This essay contained an account of his geometrical calculus in which the relative position of points is denoted by simple symbols and fixed without the help of the magnitude of lines and angles. It differs therefore from ordinary algebraic analytical geometry. The further development of this calculus was the subject of Grassmann's *Geometrische Analyse*[13] which we have already noted as being crowned by the Jablonowski Society. This was done on the recommendation of Möbius, who found in Grassmann's essay a generalization and extension of his own barycentric calculus.

We will now consider the geometrical calculus of Leibniz with a view to discovering if Grassmann's development of it has fulfilled in any way Leibniz's hopes of its ultimate importance.

§ 5.

The letters and papers of Leibniz in which he deals with his project of a geometrical calculus are many, and spread over a considerable period of time.[14] The most important is the *Characteristica geometrica,* a sketch of the notion which he made for fear it should be lost if he found himself unable to develop it. The essay enclosed in the letter to Huygens in 1679 was an extract from this. From these writings it seems clear that the starting point was his conviction of the imperfection of algebra as the logical instrument of geometry. Thus, "Algebra itself is not the true characteristic of geometry, but quite another must be found, which I am certain would be more useful than algebra for the use of geometry in the mechanical sciences. And I wonder that this has hitherto been remarked by no one. For almost all men hold algebra to be the true mathematical art of discovery, and as long as they labor under this prejudice, they will never find the true characters of other sciences." It must be noted that algebra is here used by Leibniz in its ordinary sense, not as a general term for any calculus.

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[13] This treatise develops some of the subjects which Grassmann had intended for a second part of the 1844 *Ausdehnungslehre*, which was never written.

He saw that analytical geometry only expressed geometrical facts in a complicated and roundabout manner. A figure such as the circle is not defined by its internal relations, but by reference to its relations to arbitrary coordinates. So a set of magnitudes foreign to the figure are introduced and obscure the purely geometrical relationships. Further, to reduce relations of position to relations of size presupposes Thales's theorem about similar triangles and the theorem of Pythagoras.[15] In other words analytical geometry is thus made dependent on synthetic. The analysis not being pushed far enough, it has not the logical perfection which belongs to a purely rational science.[16] He realized the want of rigor and generality of intuitive methods, but dreamed of a method which would be completely analytical and rational while still possessing all the advantages of a synthetic method.

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[14] An interesting bibliography of them together with an account of the main ideas which inspired and directed his search for a geometrical characteristic is given in Couturat, *La logique de Leibniz,* 1901.

[15] *Characteristica geometrica,* § 5.

[16] Cf. his letter to Bodenhausen.

In this his aim was similar to that which he had in mind for his universal characteristic, which was to be a logical calculus replacing concepts by combinations of signs, and which furthermore was not merely to furnish demonstrations of propositions but to be the means of discovering new ones. So, in like manner, his geometrical calculus was to combine analysis with guidance of the intuition.[17] A fusion of analysis and synthesis being made, the divorce between calculation and construction would disappear. "This new characteristic....will not fail to give at the same time the solution, construction, and geometrical demonstration, the whole in a natural manner and by an analysis."[18] It is clear that the final goal was a science of form of very wide application.[19] This aim we must distinguish carefully from the manner in which he attempted to realize it.

As Grassmann points out in the introduction to his "Geometrical Analysis," this distinction between the distant goal and his attempt toward a new characteristic which he connects with it to render the thought more realizable, is recognized fully by Leibniz. Although the characteristic he provided will be seen to be only a small first step toward the goal he had set himself, yet he had estimated the essential advantages of a final geometrical analysis to an extraordinary completeness. Grassmann says: "Just this eminent talent of Leibniz of being able to foresee in presentiment a whole series of developments without being able to work it out and without dismembering and dissecting it, yet to make it present to himself with prophetic mind and to recognize the importance of its consequences—it is just this talent which led him to such great discoveries in almost all domains of knowledge."

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[17] Leibniz conjectured that the ancients had some natural and spontaneous analysis of this kind resting on the abstract relations of figures, which underlay and helped their synthetic methods. (*De analysi situs.*)

[18] Letter to Huygens.

[19] *Ibid.* "I believe that one could handle mechanics by these means almost like geometry."

§ 6.

Leibniz founds his fundamental definitions on congruence, which means the possibility of coincidence. He represents points whose positions are known by the first letters of the alphabet, and those which are unknown or variable by the last letters. Any two combinations of corresponding points are said to be congruent if both can be brought to coincide without the mutual position of the points being changed in either of the two combinations; so that every point of one combination covers a corresponding point in the other. Congruence (geometrical equality) is a union of two relations—similarity and equality (quantitative equivalence).

All *points* are equal and similar, so all points are congruent.[20] Hence if we use $\equiv$ for congruence, the expression $a \equiv x$, where a is fixed and x is variable, is a definition of space.

It must be noticed that in defining figures by congruence the *axiom of congruence* or *free mobility*[21] must be postulated. If we do this, $ax \equiv bc$ represents a sphere of center a and radius bc.

Also, $ax \equiv bx$ represents a plane which bisects ab perpendicularly.

The above can be taken as the definition of the sphere and the plane respectively. Again $ax \equiv bx \equiv cx$ gives the locus of the center of all spheres which pass through a, b, c; and so it is a straight line.

If $ax \equiv ac$ and $bx \equiv bc$

they together give the common trace of two spheres.

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[20] *Characteristica geometrica.*

[21] See B. Russell, *Foundations of Geometry*, Cambridge, 1897.

Combined they are written $abx \equiv abc$. This therefore represents the locus of points whose distances from the points a, b are the same as the distances of c from a, b. That is, it is a circle.

The economical nature of the symbolism is shown by the fact that if we take this as a definition of the circle, it does not imply the idea of the straight line or the plane; nor does it require (as the circle defined by an algebraic equation) that the center of the circle must be known.

As an example of a proof consider the proposition that *the intersection of two planes is a straight line.*

Let $ay \equiv by$ be one plane
and $ay \equiv cy$ be the other.

Then $ay \equiv by \equiv cy$, and this we saw above to be the form of the congruences representing a straight line.

In these examples is a faint foreshadowing of the side by side development of construction, proof and analysis. And since all kinds of spatial relationships can be developed from the line and the sphere, the method is capable of wide extension.

§ 7.

There are several obvious defects in it, however. These appear at once if we attempt by means of it to solve the fundamental problem in geometry of finding the expression for a straight line passing through two given points. Leibniz had previously attacked the problem only to find himself involved in difficulties.[22]

Grassmann's treatment is as follows: We saw above the expression for a straight line was

$$ax \equiv bx \equiv cx.$$

If we now take three auxiliary points, a', b', c', which are not in a straight line, and write

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[22] Couturat gives a clear account of this, *op. cit.*, pp. 420-427.

$$\begin{cases} a'x \equiv b'x \equiv c'x \\ a'a \equiv b'a \equiv c'a \\ a'b \equiv b'b \equiv c'b, \end{cases}$$

then together these congruences represent the required straight line through *a, b,* as the locus of *x.*
Combining the last two we get

$$\begin{cases} a'x \equiv b'x \equiv c'x \\ aba' \equiv abb' \equiv abc'. \end{cases}$$

This then expresses that the auxiliary points lie on the circle the plane of which is cut at its center by the line *ab* at right angles.

If this expression is to have the necessary simplicity, it must be possible to eliminate the arbitrary auxiliary points which have nothing to do with the nature of the problem, and to combine the group of formulas into one. That being impossible, the characteristic has failed to serve its purpose.

Indeed the failure of the method followed at once from the choice of congruence as the fundamental relation. For, as we have seen, this complex relation contains a quantitative element, and so prevents any freeing of geometry from considerations of magnitude. In fact, as the above expression for the line through *ab* shows, we are still left with arbitrary coordinates. Further, in this system there is also ambiguity, as Couturat has shown.[23] In other words the analysis had not gone far enough. If what remained of magnitude had been eliminated—not merely by taking the relation of similarity, for Leibniz had himself shown that to imply metrical relations[24]—but by reducing figures to their projective properties and relations, at least a real geometry of position[25] would have followed. But such a projective geometry, while satisfying Leibniz's desire to eliminate algebraic methods from geometry, would not have been a geometrical calculus with points as elements. Nor could it have had the wide application which he sought for in his calculus; for if it was to be applicable to mechanics and physics, it must at some point have been susceptible of metrical development.

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[23] *Ibid.*, p. 428, note 2.

[24] *"Elementa Nova Matheseos Universalis."*

[25] Developed by Staudt, *Geometrie der Lage,* 1847.

Now, throughout our discussion we have seen that Leibniz was seeking for a characteristic particularly applicable to geometry but akin to his universal characteristic. At the end of the letter to Huygens he says: "I believe it is possible to extend the characteristic to things which are not subject to imagination." In other words he was seeking a formal calculus, an abstract mathematics lying at the base of geometry and applicable not only to it but also to logic. Now Grassmann had already developed such a science of form in his *Ausdehnungslehre* of 1844. So when the Jablonowski Society announced the subject of their prize essay he took the opportunity of expounding his science of extensive magnitudes, not as he had originally derived it, but starting from Leibniz's characteristic.

§ 8.

When he had proved the insufficiency of the relation of congruence as Leibniz had left it, he tried to give it a form in which substitution would be possible. What are congruent to the same thing are congruent to each other, but that does not mean that we can in a general way place instead of a given term in a congruent expression one congruent to it. So substitution is not possible. This can be seen at once. All points are congruent. Therefore, if one could substitute the congruent, one could place *abc* congruent to every combination of three points—which is absurd.

Grassmann rightly regarded the fact that substitution was not possible as a serious defect in the calculus. So he inquired what equations would hold between the points a, b, c, d, e, f, if $abc \equiv def$.

There must be some function f such that, when the above holds,

$$f(a, b, c) \equiv f(d, e, f).$$

So he was led to the general linear relation of collinearity.

Now in the *Ausdehnungslehre* Grassmann had reached the fruitful idea of a true geometrical multiplication which has the peculiarity that if any two factors of the product are interchanged the sign of the product is changed, that is,

$$\mathrm{AB} = -\mathrm{BA}.$$

This *combinatory* multiplication enabled him now to give an intrinsic definition of geometrical figures in terms of points, and so to accomplish what Leibniz had failed to do. Thus the product ab determines the straight line between the points a, b; the product of three points determines the plane, and so on. But since the product is non-commutative these figures when so defined have a sense represented by the signs $+$ or $-$. Furthermore, he conceived the notion of using these products to express not only relations of position but also of magnitude. So that the same analysis which gave a geometry of position also gave, side by side and without confusion, a metrical geometry. In making this step he had to define (§ 3, *Geometrische Analyse*) what he meant by a point magnitude. Each element (point, line, plane) has two aspects—its position in space, and its intensity. In the case of the point, this latter was represented by a positive or negative "mass."

By now defining a *line magnitude* as the combination ab of the point magnitudes a, b— the direction of which is through a and b, and the intensity of which can be defined; and also defining the *point magnitude* as the combination AB of two line magnitudes, the position of which is the intersecting point of A and B and the mass value of which can be made the subject of a definition—then by an assumption which makes $ab = O$ and $AB = O$ represent coincident lines and points, it is possible to write in the form of an equation every linear dependence.

Thus $(ab)(cd)e = O$ denotes that e is the intersecting point of ab and cd.

So the principle of collineation can be expressed, though cumbrously without further adaptation, by such combination equations.

In this way *equality* is made to include the two relations of identity of position and equality of intensity. So projective and metrical relations can be expressed in one form, and considered either separately or together.

§ 9.

It is impossible to follow Grassmann's development[26] further without setting up a technical symbolism, but it may easily be shown how brilliantly Leibniz's hopes of an analysis specially applicable to mechanics have been fulfilled.

In terms of this calculus the sum of n points is their mean point. If intensities are considered, the metrical relation follows. Thus if the intensities represent masses at the points the sum gives the center of gravity of the system—a point whose intensity will be the sum of the other intensities. If the intensities represent parallel forces acting at the point the sum gives the point of application of the resultant. The *barycentric calculus* of Möbius is thus included in this more general analysis.

Furthermore, the line magnitude of Grassmann expresses a force with exactitude. Composition of forces thus becomes the addition of line magnitudes. The general equations of dynamics can also be represented (§ 11, *Geo-*

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[26] Needless to say the above is a mere sketch of the beginning of Grassmann's "Analysis." In particular no mention is made of his distinction between *inner* and *outer* products.

metrische Analyse) by means of this calculus, as soon as certain modes of treating infinitesimals have been evolved.

Moreover the possibility of attaching a metrical coefficient to each point in space opens at once many fields of application in physics.

We must notice in addition that the "Geometrical Analysis" does not treat of the *quotients* of non-parallel stretches, a subject which leads to a calculus for dealing with powers, roots, logarithms and angles.

Grassmann can claim justly therefore, as he does, in the concluding remarks to this work, that his mode of treatment, if transferred to physics in general, would simplify the mathematical treatment in a splendid manner. He himself has shown the great advantages of the calculus in many fields. In the essay we have several times referred to, Leibniz wrote, "If it [the characteristic] were set up in the manner I conceive, one could construct in symbols, which would only be the letters of the alphabet, the description of any machine.... One could by these means make exact descriptions of natural objects."

As an example of such descriptive power Grassmann mentions his application of the calculus to crystallography (cf. *Ausdehnungslehre* of 1844, § 171).

§ 10.

Apart from the adaptability of the geometrical calculus to different provinces, there are other good reasons for believing that it realizes the ideal toward which Leibniz looked forward.[27] Grassmann's claim put forward in his concluding remarks will, I think, be granted by any one willing to master the symbolism sufficiently to understand any of his theorems. "As in the analysis demonstrated here every equation is only the expression, clothed in the form of the analysis, of a geometrical relation, and this relation expresses itself clearly in the equation without being obscured by arbitrary magnitudes—as for example the coordinates of the usual analysis—and therefore can be read off from it without further trouble; and as further every form of such equation is only the expression of a corresponding construction, then it follows that as a matter of fact, by means of the analysis here given, the solution of a geometrical problem results at the same time as the construction and the proof. As further nothing arbitrary . . . . need be introduced, the kind of solution must always be according to the nature of the problem; and as it is in the form of analysis, therefore a necessary one in which there can be no question of any seeking round for methods of solution." In other words the fusion of synthetic and analytic methods which Leibniz hoped for is fully accomplished.

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[27] Letter to Huygens: "Algebra is nothing but the characteristic of indeterminate numbers, or of magnitude. But it does not express exactly situation, angles and movement.... But this new characteristic....cannot fail to give at the same time the solution, the construction and the geometrical proof, the whole in a natural manner and by analysis. That is by determined ways."

It must be noted that in one respect Grassmann has not only realized the prophetic vision of Leibniz but also cleared away the inconsistency which vitiates his attempt at making his dream come true. For Leibniz, seeing that the *fundamental* analysis of geometry must rest on non-metrical relations, yet desired its *final* application to mechanics and natural science, in which metrical relations are all important. So he was led to a half-hearted attempt at non-metrical analysis by means of a relation—congruence—which, while showing the way to a geometry not based on algebra, yet failed itself to travel far in that direction. The special merit of Grassmann has been to found a geometrical analysis free of magnitude and yet so to develop it that metrical considerations may be introduced without disturbing the form of that analysis. Projective geometry, therefore, only partly fulfils Leibniz's hopes;

their complete realization is found in Grassmann's theory of extension.

§ 11.

We began our discussion of the relation between Grassmann's calculus and the characteristic of Leibniz by an analysis of the manner in which their work has been received by the average mathematician. It seems to me that we can profitably return to these historical considerations for a moment, and look at them from another view-point.

There is some reason, as I have tried to show elsewhere,[28] for citing lack of historical perspective on the part of mathematicians as the cause of the unsympathetic attitude commonly taken up in regard to work of philosophical breadth; and that if more regard were paid to historical development in mathematical education wider and more penetrating vision would result. The position taken up is well expressed by Branford[29]: "The path of most effective development of knowledge and power in the individual coincides, in broad outline, with the path historically traversed by the race in developing that particular kind of knowledge and power." At the same time, however, we must realize that, if we alter our attitude to this slightly, and regard it not from the point of view of the educationalist but from that of the original worker himself, obsession with origins seems inevitably to lead to what I have called above the genetic error. The effective point of departure in attaining knowledge of geometry may be from such empirical and utilitarian experiments as form its historical origin. But that must not be allowed to create an atmosphere hostile to any recognition of the *a priori* and formal nature of that science.

Furthermore the historical method may lead to a certain *ex cathedra* manner, a reliance on authority and tradition. It is this factor which especially concerns us in our attempt to see the work of Leibniz and Grassmann in true relation to each other and to mathematical thought. For Couturat points out[30] that what probably hindered Leibniz's development of his geometrical calculus and rendered abortive his attempt at its realization was the authority of Euclid. He says, "Why, amidst all the relations which Leibniz catalogued, did he give preference to the relation of congruence and neglect the relations of similarity, inclusion, situation, which serve to-day as the bases of quite new sciences[31] which he foresaw and would have been able to found? It is evidently because tradition, represented and embodied by the *Elements* of Euclid, limited geometry to the study of the metrical properties of space. Now the tradition is not explicable by any reason of *theoretical* order (considering that metrical relations are more complex and less general than projective relations) but solely by reason of historical and practical order."

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[28] "The Neglect of the Work of H. Grassmann."

[29] B. Branford, *A Study of Mathematical Education*, 1908.

I have already in the previous section shown that another explanation may be held of this clinging to a metrical relation by Leibniz. However that may be, the authority of the Euclidean tradition may have had some influence on his work in geometry, as the Aristotelian tradition had in his foreshadowing of a logical characteristic.[32] In fact we shall not be laying over-emphasis on the tendencies of an exaggerated reliance on historical method if we say that its final result is the attitude of the young critic in Shaw's play[33] who says, in effect, "Give me the name of the author and I'll tell you if it's a good play." If that critic held a university chair of historical criticism he would doubtless be able to find valid arguments for his position—for how

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[30] *La logique de Leibniz*, pp. 438-440. Russell however attributes Leibniz's failure to his holding the relational theory of space, *Mind*, 1903, p. 190.

[31] Theory of aggregates, modern *Analysis situs*, projective geometry, etc.

[32] See note 7 above.

[33] "Fanny's First Play."

(he might ask) can one judge competently without a complete set of data, and is not authorship an important datum? It is irritation at this standpoint which causes Mr. Bertrand Russell, whom I have heard speak very forcibly on the subject, inveigh against this hyper-historical method. But the objection can be stated in a much stronger form. "Erudition often does violence to inventive power: and the proof is that the modern discoverers of symbolic logic, Boole and his successors, have all ignored (and rightly) the example and precedent of Leibniz; it has even been remarked[34] that they have almost all been ignorant of one another, and if this ignorance has been a source of error, it has been above all a condition of originality."[35] Now it does not appear to me that the essential defect of such an extreme anti-historical attitude has been that it caused error. Staudt realized the ambitions of Leibniz in some degree in founding his projective geometry, and Grassmann in still further degree in creating his theory of extension, without knowing that their historical origins lay in his work. No great harm comes from this, although an original genius will, as a general rule, be less likely to be deflected from his way by the work of others than to find in them sources of stimulation. But to the mass of us, who form the bulk of mankind, narrowness is a mental blinker which hides the full splendor of the creations of genius. The real toll taken by historical ignorance is in neglect of originality, and the loss of power and influence consequent on it.[36]

A. E. Heath.

Bedales, Petersfield, England.

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[34] J. Venn, *Symbolic Logic,* Introd., pp.29, 30.

[35] Couturat, *op. cit.,* p. 440.

[36] I have to thank Miss Vinvela Cummin for valuable help in translating the *Geometrische Analyse.*

GREEK IDEAS OF AN AFTERWORLD.

A STUDY OF THE RELATION BETWEEN PRACTICE AND BELIEF.

WE have a free and easy way of generalizing the afterworld of Greek religious belief as an underworld. This is indeed the usual form of the belief from Hesiod onward, and it is the view generally disclosed by Homer both in the Iliad and Odyssey. Yet the fact is that the most deliberate and detailed Greek presentation of the approach to that dread world, that of the eleventh book of the Odyssey, does not at all represent it as an underworld like the infernal regions of Vergil's fancy, but as a far western realm. The far-wandering Odysseus sails to the distant west, out of the sea, and across the mighty ocean stream to its farther shore; he beaches his black-hulled ship on a lone waste beach where stand the barren groves of Persephone; thence he directs his steps inland to a great white rock at the confluence of the Styx, Pyriphlegethon and Acheron; and there it is that he enters the purlieus of the many-peopled house of dark-browed Hades.

The Odyssean realm of the dead is reached neither by descent into a cave nor by passage underneath an overhanging ledge. It is of the same level as the land of living men. Its darkness is apparently due to its location beyond the path of the western sun, which, descending into Ocean Stream, disappears somewhere from the sight of mortal men to be ushered in anew by rosy-fingered Eos, each succeeding morn. To speak of Odysseus as descending into an underworld is to have but little regard for the language of Homer. Clearly to discern the picture that he actually presents is to become aware of a striking contrast between it and the afterworld of classic Greek and Roman belief; and this contrast raises the problem of explaining and accounting for such different views, obviously related in the same way to the same fact,—the fact of death. An obvious relation, I say; if this appears to be but a bold assumption, I trust it will be justified in the course of my argument.

A study of early man's beliefs about an afterworld involves a consideration of two groups or series of facts—mental facts and motor facts, or facts of belief and facts of practice,—both associated with the event of death. Apparently these two kinds of facts do not simply constitute two parallel series that were mutually unrelated in life and thought and that may therefore be studied and understood apart from each other; they seem to have an intimate and genetic relationship. This, however, is not to say that they are absolutely simultaneous in origin, or that one may not be primary and the other secondary, both in origin and importance. On the contrary, in their genesis, either belief is antecedent and causal to practice, or practice is antecedent and causal to belief.

It is popularly supposed that belief originates and dictates practice, or custom, which is thus regarded as secondary to belief. Anthropologists generally confirm the supposition, and whole systems of social interpretation and philosophy have been built upon the assumption. Professor Seymour, in his *Greek Life in the Homeric Age,* insists upon this relation in the case of Greek mortuary practice and belief, and cautions the reader against assuming that the Greeks who maintained certain customs may have "inherited also the beliefs on which those customs were originally based." He brings to bear upon the case the authority of the German scholar Rohde, declaring that "Rohde gives as the *cause* of the adoption of cremation by the ancestors of the Homeric Greeks, a desire to rid themselves of the souls of the dead; and as a *result* of the change, the abandonment of the old ritual and sacrifices."

According to Professor Brinton, "The funeral or mortuary ceremonies, which are often so elaborate and so punctiliously performed in savage tribes, have a twofold purpose. They are equally for the benefit of the individual and for that of the community. If they are neglected or inadequately conducted, the restless spirit of the departed cannot reach the realm of joyous peace, and therefore returns to lurk about his former home and to plague the survivors for their carelessness.

"It was therefore to lay the ghost, to avoid the anger of the disembodied spirit, that the living instituted and performed the burial ceremonies; while it became to the interest of the individual to provide for it that those rites should be carried out which would conduct his own soul to the abode of the blest."

Here again practice is regarded as secondary to belief, and is interpreted by reference to belief. Professor Frazer, also, the dean of living anthropologists, insists upon this relation between our two series of facts, and cannot admit or conceive of the opposite as being true. I intend, however, to take the other side of the question here involved, advancing the proposition that it was mortuary practice that constituted the motive for belief in an afterworld; and especially shall I endeavor to indicate the application of this formula to the genetic interpretation of Greek ideas of an afterworld.

The Hellenic peoples of whom we have knowledge universally believed in an afterworld, whither the souls of mortals departed at death and where they had a continued existence. But they entertained not merely the two conflicting beliefs already mentioned; they held in developed form at least four quite different beliefs regarding the destination and abode of the souls of the dead. According to one of these beliefs the souls of dead men ascended to Olympus, as did that of Heracles in story; according to another they descended into an underworld; in the eleventh book of the Odyssey Homer places them in a continental region beyond the western verge of Ocean Stream; and Pindar places the souls of great heroes in "Islands of the Blest" in the far Western Ocean.

It may be well at this point to note some apparently fundamental resemblances between these last two beliefs. Pindar places the souls of sinful mortals in an underworld, subject to sentences reluctantly imposed upon them. Hesiod declares that the men of the Golden, Silver and Bronze ages were hidden away in earth; and it is but natural, because of the different types of life imputed to them, that he should fancy different conditions for them after death. But the souls of his age of heroes, he says, were given a life and an abode apart from men, and established at the ends of the earth in "Islands of the Blest by deep-eddying Ocean." He does not state the direction of these wondrous islands, but undoubtedly their direction, like that of the Pindaric Islands of the Blest and that of the Odyssean realm, was already so fixed in the tradition of his day that there was no need of indicating it. It would appear, then, that in essential characteristics the continental Odyssean realm and the Islands of the Blest are alike in being conceived as western, and differ only in geographical form and extent. From this it would further appear that the notions of these two similar abodes of the dead are variants derived from a single source. But if these two notions did grow out of a single origin there was certainly a reason for the divergence, which it should be part of our task to discover. And yet, on the other hand, it may be unnecessary or even incorrect to assign their origin to the same people, even though we may feel compelled to assume that the significant common element of direction must inhere in a common element of antecedent cause.

Whence came these three or four differing beliefs? That is to say, upon what difference of psychological ground do they severally stand? No one man could at one time entertain so many and so contradictory beliefs upon one subject; neither could one homogeneous people, as, for example, a single city state of the Mycenean civilization, or even the Minoan civilization of Crete as a whole. Wherefore we should probably look for this difference of belief either in the several racial stocks amalgamated to form the historic Greek people, or in part to their respective traditional beliefs and in part to alien streams of influence. But in either case it will be pertinent to inquire how different races and racial stocks should have come thus to act and believe so differently in the face of the same fact, death. To trace a belief or practice from one people back to another should never be taken as an explanation; this done, the question of real origin and motive still remains, as insistent as ever. Neither should identity of belief or practice be taken as necessary evidence of racial relationships, or of racial contacts; nor difference of belief or practice as evidence of difference of race. There are others besides the human factor that enter into the origin and development of practice, as we shall presently see.

With regard to mortuary practice, the Greek world furnishes only two types of historically attested facts. The Homeric Achæans cremated their dead, and the practice survived far beyond the Homeric, and even the Periclean age. The Mycenean civilization laid its dead beneath the surface of the earth, and this practice gradually superseded cremation, even among the descendants of the Achæans. Thus the Greeks of historical times had two strongly contrasted modes of disposing of their dead, corresponding to two of the contrasted beliefs we have mentioned. For there is undoubtedly a genetic relation between cremation and belief in a heavenly abode of souls, and between inhumation and belief in an underworld. But which is cause and which effect? And how did the causal series itself originate? And how could the belief in a western abode of souls be related to either of these, either as antecedent or as consequent?

These two series of facts in Hellenic life give rise to three problems of immediate significance; to say nothing of others more remote, as for example, how man came to believe that he had a soul at all, how nearly the belief coincides with actuality, the origin of religious fears, etc. The three special problems thus isolated for present consideration are:

1. What is the genetic relation and order of precedence between practice and belief,—between cremation and belief in a heavenly abode of souls, and between inhumation and belief in an underworld?

2. In case either belief or practice is found to be antecedent to the other, how then did this antecedent series take its rise?

3. Whence and how came the belief in a far western abode of souls, and why the apparently twofold differentiation of this belief, which we have noted?

In the interest of brevity I may appear to be cutting the Gordian knot rather than untying it; but I feel sure that the drift of my argument will be caught, and that its essential truth must make a strong appeal for assent.

In the first place, let us consider this intimate and inherent correspondence between mortuary practice and belief about the dead, under conditions where we can see more plainly the part played by geographical environment, and where at the same time we can be sure of the soil on which our two series of facts originated; for we know not yet where the practice of inhumation originated among the

Myceneans and Minoans, nor where cremation first developed among the Achæans.

The ancient Egyptians and the Incas of Peru preserved their dead by mummification, and both believed in a bodily resurrection of the dead. We are reasonably sure that the land where each of these peoples developed was likewise the soil upon which their respective traditions in this matter originated; we shall be still more sure of this local origin as we proceed. Did the belief or the practice precede?

Now no matter what we may imagine them to have thought about soul and body and their mutual relations before the practice began, the Egyptians and Peruvians could not have cremated their dead; both Egypt and Peru lacked that abundant supply of fuel which would be necessary for this practice among a numerous people. Neither could either people long have inhumed its dead in the fertile valley land of its abode. These restricted valleys early became the seat of such dense populations that productive land could not be permanently set aside for burial purposes; nor could land under cultivation be wantonly trampled over for this common social purpose, even though six feet of earth were sufficient for the individual grave. Of necessity, therefore, the adjacent desert ridges were employed for the purpose, and the earliest mode of burial there was inhumation. But the dry climate and the nitrous character of the upland soil, both in Egypt and Peru, tended naturally to preserve the bodies of the dead. The action of wind and wild animals, however, tended often to exhume them, at the same time disclosing a high degree of preservation. In order to protect their dead, especially to prevent the work of their hands from being made of none effect, the Egyptians, in particular, came to build rock tombs. But this required much labor and expense. Yet it was cheaper to build one tomb large enough for many burials, for whole families, even through successive generations, than to build many individual tombs. Hence, by mutual suggestion and social rivalry through long stretches of time, the mighty Pyramids of Egypt came to be developed.

But under these conditions a tomb must be entered from time to time for new burials; and in spite of their high degree of preservation by natural means, the bodies of the dead within gave rise to noisome odors. Hence arose the practice of embalming with aromatic spices, to counteract or obscure the evil odors of decomposition. What but this fact of unpleasant odors could first have suggested the use of expensive spices in embalming? With the prominent Egyptian nose was undoubtedly associated a keen sense of smell. Wrappings of linen served in the first instance to retain the spices. The embalming tended to more perfect preservation of the flesh, and this result also helped to accomplish the primary object of the practice, which was the laying of unpleasant odors. Upon this combination of facts arose a profession of embalmers, who developed a more and more elaborate technique. When death and funerals had thus become an economic burden upon the living, for which no obvious or adequate return was received, the question of meaning inevitably arose and persistently pressed for a satisfactory answer. It is exceedingly difficult for man to admit that he is spending sacred energies in vain or purposeless quests, and thus making a fool of himself; and so the practice, entailing so large an expense, insistently required a sanction, and a tremendous one at that.

Now the care lavished upon the dead body, by tending to preserve it for an indefinite length of time, embodied within it an inherent and obvious suggestion of the primary sanction that actually came to be formulated. For by this time embalming had come to take place before the process of decomposition had set in; and the original cause of the practice was no longer making its appearance, even though from allied experience the agents may well have been aware of what would soon happen without embalming and burial. So now, instead of really knowing that they are trying to forestall or allay the noisome odors of decomposition, they detect but one purpose in the practice, the preservation of the body. But why should the body of the dead be preserved? With this query arose the first suggestion of a mystical or transcendental idea in association with the practice, and the first attempt to formulate an ultra-pragmatic or other-world sanction for it. This sanction was formulated as an explanation. It was from the first employed for this purpose, and as all thinking individuals were implicated in the practice no one was in a position to question or challenge it.

It might be urged on this latter ground that the question of purpose or value could never have arisen; but we must not overlook the fact of foreign contacts—especially among the Egyptians—wherein contrasted practices would raise the question from without, if not from within. Besides this, they had always the poor with them, who, from contrast with their own meager efforts in the same field, would be forced to think about values. And above all, there was always growing up among them the supreme pragmatist,—the eager, curious child.

Thus this question of values, like the ghost of Banquo, was ever likely to confront the living, and only a powerful sanction would serve to lay it. The priesthood and the professional embalmers, in particular, had constant need of the sanction, as a means of justifying their existence. Thus it is that this sanction arose, and that it has been passed on and received as an explanation even by the wisest, even unto the present day. And that is in brief the story of the Egyptian and Peruvian practice of mummification, and of their belief in a bodily resurrection. It all comes back in the last analysis to the fact of decomposition and the despised sense of smell, which would move men to acts of aversion and riddance.

But, one may ask, is it not after all just possible that this practice arose out of an antecedent idea of souls and the notion that the body must be preserved against a future resurrection and a reincarnation of the soul? Rather is it not far more reasonable to see that the belief arose out of the practice, as a sanction for the care and expense involved in it? On the first alternative we must certainly congratulate the Egyptians, and the Peruvians too, on having found a geographical location so congenial to their belief. What would they have practiced, or how could this belief have survived, had they lived in the valley of the Congo or Amazon, or even in Greece? Or how could they have come to believe in a heavenly abode of souls, when they did not cremate? And if the belief in a bodily resurrection came before the practice of mummification, then how did this notion and belief arise?

Now let us take a look at barren, hungry, frost-bitten Tibet. What burial practices and what cognate beliefs about the dead have from the first been inherent in the natural environment of man presented by the Himalayan highland? Let us picture to ourselves a people making here its arduous ascent from lowest savagery to barbarism. As they come to have a settled place of abode, how shall they secure for themselves riddance from the discomforting odors of decomposition that follow in the train of death? Suppose that they have attained to such a degree of economic efficiency as to have left behind the practice of cannibalism, and that they are as yet without any metaphysical or transcendental ideas and beliefs; how then shall they dispose of their dead? Or what shall they believe about their dead, if they have as yet paid no attention to them save by the simplest modes of seposition and abandonment?

Here in Tibet is a people that could not cremate its dead; for here, too, fuel is scarce. Neither could it inhume its dead; for during a considerable portion of the year the deeply frozen ground is proof against even the tools of civilized man. Here preservation of the dead by natural means, that of freezing, may be assured for a season; but should this be relied upon temporarily, final burial by one means or another would become imperative with the advent of spring. Shall the Tibetans preserve the bodies of their dead through the long winter, to the end that they may give them some sort of approved burial in the spring? What could originally have suggested to them the notion of an approved form of burial, and of the preservation of their dead against the time when this should become possible? The primary function of burial by whatever means is avoidance or riddance of certain after effects of death; and with an abundance of carnivorous animal life scouring the country for the means of subsistence, how could the immediate, practical function of burial be more readily or more easily secured than by calling in the aid of dogs and vultures that infest the land? Now this is exactly what the Tibetans do, even to-day. And from their own hard struggle for existence they furthermore feel it an act of charity thus to minister to these scavengers of their land. There is no other people on earth with whom charity is so highly esteemed as a virtue, and so universally encouraged. Under the hard conditions of life, charity, generosity, is a necessary practice among their own kind. And furthermore, the leisure-class priesthood, which is very numerous, in its own self-interest has need of encouraging this fundamentally necessary virtue; and finally, this virtue is invoked as a sanction for the feeding of their dead to the beasts of the field and the birds of the air. Without some notion of other ways of securing this same object, they could feel no need of this or of any other sanction.

In the case of a very few individuals of the highest rank cremation is allowed as a special honor, and naturally this privilege is mostly restricted to the religious hierarchy. It is evidently not the native Tibetan practice, but was plainly introduced into Tibet by the Buddhists of India, with whom it was native. But the great majority of the Tibetan dead go to feed the hungry dogs and vultures, which are highly esteemed for this purpose; and this, despite the fact that Tibet has for a dozen centuries been subjected to Buddhist influence, which would naturally favor cremation, its own native mode of burial, if this were economically possible. Here in Tibet the native mode of burial is directly apposite to geographical conditions, even as it was in Egypt and Peru; and the beliefs by which it is explained are merely so many sanctions, or justifications, which have been developed out of the practice itself.

But what are the Tibetan beliefs about the dead? When once they have acquired the notion of a soul that survives the event of death, whether originally or by adoption from other peoples, we should expect them to hold a belief in some sort of transmigration. From seeing the bodies of the dead devoured by animals, they would seem naturally to think that souls also passed into the bodies of these living sepulchers. This is exactly what they believe. We should furthermore expect them to have a preference for transmigration into the winged vulture that sails so easily through the air, to taking up their abode within the body of a lazy, grunting pig or snarling dog. Here too our surmises are correct. In the course of centuries, as the relation between practice and belief has become obscured, their beliefs have been elaborated and graduated, so that even non-carnivorous animals are included in certain cycles of transmigration. But in this fact of feeding their dead to animals is certainly to be found the original germ and suggestion of their belief in transmigration. Tibetan religious ideas and beliefs are not so definitely conceived nor so systematically organized as are those of some other peoples, because their authors have never devoted so much personal care and energy to the disposal of their dead. They have not felt so strong a necessity for justifying their practice as have the Egyptians and some other peoples.

Again, let us consider the case of India, where Brahmanism and Buddhism have their origin and home. The Indians, like the Tibetans, hold a belief in transmigration, and of course for that same fundamental reason. That a mighty, far-scattered people like the Indians exhibits a characteristic belief or practice does not mean that all individuals of the group hold it in common. It would be too much to expect such a people, or any people at all, to be really homogeneous in belief and practice from the early stage when human burial began among their forebears until the present time. Thousands of families in India today are too poor to afford the most characteristic traditional form of burial for their dead, and throw them into rivers, or otherwise dispose of them. In Indo-China those to poor to afford cremation commonly carry out their dead to be eaten by the beasts of the jungle. On the Ganges, "When the pyre is built the nearest relative of the deceased goes to the temple and haggles with the keeper of the sacred fire over the price of a spark; and having paid what is required he brings the fire down in smouldering straw and lights the pile. If the family can afford to buy enough wood, the body is completely consumed; in any case the ashes or whatever is left on the exhaustion of the fire is thrown into the sacred river;. . . .and any failure on the part of the fire to do its full duty is made good by the fish and the crocodiles."[1] Thus it is easy to see how in bygone days the Indian, at least in the lower social strata, became possessed of a belief in transmigration, and how, through ignorance of its primary source and relationships, carried it over into relationships bearing little or no connection with its parent practice, as in his abstinence from eating flesh.

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[1] Pratt, *India and its Faiths.* New York, 1915, p. 44.

And yet India, with its wide extent and countless population, has more constant elements of religious and philosophical belief than would at first seem possible,—a result of mutual contacts and social cooperation through long stretches of time. "The central point of Hindu thought is the soul. It is from the soul or self that all the reasoning of the Hindu starts and to it that all his arguments finally return."[2] Probably the most widely known characteristic of Indian religious philosophy is the doctrine of the immanence and absoluteness of the supreme soul Brahman, with its correlate doctrine of the oneness of the individual self with the All,—the merging of the objective, phenomenal world into the universal absolute, which is Brahman. Yet it is plain that this interest in the objective world begins with the individual human self. "This unity of the soul with God is at the foundation not only of Hindu metaphysics, but of Hindu ethics as well. The great aim of life is the full realization of that God-consciousness, the significance of which forms the central point of Hindu thought. Before this can be fully attained, the soul must be liberated from the mass of particular interest and petty wishes and self-born illusions which weigh it down and hide from it the beatific vision. Hence *liberation* and *realization* may be called the twin ideals of Hinduism, and it is these that determine all its ethical theory."[3]

The doctrine of "liberation" and "realization," the doctrine of Nirvana, the yoga-systems, and other characteristic Indian notions would be meaningless and impossible without the basic body of "religious intuitions" that make up the Brahmanistic doctrine of the Upanishads. But an "intuition" has in primordial genesis some sensuous basis, direct or indirect; and so, instead of seeking for the idea or philosophy back of the practices associated with these and other beliefs, we should undoubtedly seek in practice for the sensuous elements of suggestion that formed the basis of the beliefs, and then seek in turn for the sensuous motive of the practice itself.

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[2] Pratt, p. 91.

[3] Pratt, p. 92.

We may admit that the Indians are a peculiar people; yet, when we pin ourselves down to minute details, we note that the testimony of their senses, the ultimate constituent of all intellectual forms, is the same as our own. Their intellectual peculiarity consists not in their physical or psychological selves, but in the differences of their objective environment, part of which they themselves make, and in the various ways in which the sensuous details of experience with it have been combined through generations of spontaneous social collaboration.

If, then, we consider these doctrines of the "infinite ocean of the absolute Brahman"; of the essential oneness of the one with the All; of the soul's struggle for liberation to realize and complete this oneness in "Nirvana, or reabsorption into the eternal light": as we contemplate these doctrines, seeking to discover their source in sensuous experience at a time antedating the rise of science with its theories of atoms and corpuscles, can we not almost see before our eyes the primitive populace of India cremating its dead and beholding the body ascending in the form of flame and smoke, thus becoming absorbed in the ocean of air, which to them, at that time, seems infinite?

We examine Indian burial practices, both present and past, and we find that from time immemorial cremation has been a characteristic Indian mode of burial. When men actually beheld the body of a deceased friend dissolve and mingle with the elements, they were bound to have different thoughts about the destiny of the individual than if it were laid away in earth to decompose by degrees for an unknown length of time, or if it were altogether preserved by embalming against decomposition. And from seeing the individual thus pass so visibly from a corporate existence into thin air, they would also be moved more strongly to contemplate the other end of individual existence, the whence as well as the whither. There could be no doubt that the deceased had attained to freedom from the bonds and ills of terrestrial existence; and the living, from their own desires to live beyond the usual limits of life, would be brought face to face with the question whether they should ever live again and how their scattered selves could realize another conscious existence. To hold before them the notion of another life as something to be desired was to believe in it; and from this point it was an easy matter to identify the conditions of existence before birth and after death, whence Brahman becomes the source, the end, and the essential constituent of individual existence.

Add to all this the practice of feeding to animals either the entire body or the remains of partial cremation, already noted—the differences of practice being characteristically in agreement with differences of social rank—and we have the proper sensuous background in practice for the doctrine of transmigration, which we find embodied in the doctrine of Karma and fused with the doctrine of Nirvana.

Geographical conditions undoubtedly favored cremation in India in the days when fuel was abundant and easily secured. But with a numerous population making large demands upon the wood-supply through scores of generations, the practice has become more and more expensive, and the demand for sufficient sanction has become more imperative. Thus in the course of many centuries the beliefs genetically inhering in these practices have become much elaborated; and, by the development of an elaborate logic and metaphysic, they have in turn modified the practice itself. It is in this way that the religious institution has justified its ways and made itself indispensable to men.

Here again we may claim without fear of successful contradiction that burial practice arose as a purely practical matter and by its form dictated the form that belief about souls must take, when once the notion of soul itself arose out of the practice. The sense of smell together with the simple, practical knowledge of the purifying agency of fire suggested and motivated the practice; here it is that we find the sensuous motive behind the practice, which in turn motivated the belief. Primarily, the belief is a supposed explanation of the practice, invented when the practice had become so highly elaborated as to conceal its real cause and thus to demand justification. Men do not feel the need of explaining or justifying the obviously practical.

But the explanation given of this and other kinds of practice is not an explanatiaon of the covert act; rather is it intended to explain or justify the care and energy devoted to it or required by it in the name of social form. The overt act merely affords suggestions toward the explanation that is evolved. It is only after a long lapse of time during which a practice has by social concurrence become highly elaborated that a justification is required. Men acting in unison, with a common sense or emotional interest, will do extravagant things not dreamed of in individual life. But, having participated in such an act, unsophisticated man can easily find a justification for his act, suggested by the act itself. It seems to be a characteristic of universal human nature, in the absence of a true, antecedent cause for specified conduct, to seek about for some consequent justification; and the race seems equally prone to accept such a justification as a statement of antecedent cause.

And now we may return to the case of Greece. We do not find there that close, almost necessary relation between practice and environment which we have seen in Tibet, Egypt and Peru; in fact, we cannot say with certainty where the two historic Greek forms of burial originated. Already some 3000 years before the Christian era we find the Minoan civilization in the Ægean world, practicing inhumation. And the northern Achæans, from whatever source they came, were already at their arrival in Greece practicing cremation. As to the relation between the beliefs and practices that prevailed on Hellenic soil, we can argue only by analogy, or homology, with what we have seen to be true in Egypt, Peru, Tibet and India; but it is far more reasonable to believe that the same relation holds true here than to defend the other horn of the dilemma.

With regard to the Achæan belief in a heavenly abode of souls, we may cut the matter short by asserting its rise out of the practice of cremation. In the course of time, after this practice had become the rule among the ancestors of the Homeric Achæans, they probably came to feel much the same regarding it as did the Indian of California. "It is the one passion of his superstition to think of the soul of his departed friend as set free, and purified by the flames; not bound to the mouldering body, but borne up on the soft clouds of smoke toward the beautiful sun."[4] I say the Achæan may have come to feel in this way, much as did the Hindu; but this was not the original motive of his practice. His thoughts about the mouldering body of his departed friend and his fancies about purification were not in the first instance inspired by a desire for the friend's welfare after death; he was first of all concerned for the living, especially with regard to the sense of smell. And however transcendental the notion of purification came to be by reinterpretation of the practice, after its original motive had ceased to prevail—because burial came to be practiced before decomposition had set in—the very association of purity with cremation betrays the original motive of the practice, just as did the use of spices by the Egyptians.

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[4] Powers, *The Indians of California*, pp. 181, 207.

As with cremation among the Achæans, so in the case of inhumation among the Minoans and Mycenæans we may assert that the practice was suggested, and passed through its primary stage of development, as a means of escape from the discomforting odors of decomposition. And as the belief in an upper-world abode of souls developed as an explanation and sanction for cremation, so belief in an underworld developed by suggestion from the practice of inhumation. To make good the claim that belief came first and suggested practice, one must show satisfactorily how any people ever could have associated souls with a heavenly or with an underworld abode without the practice of cremation or inhumation, respectively, or at least contact with some people who did practice this mode of burial.

The belief associated with cremation never became so highly elaborated in Greece as it did in India, and for very good reasons. For in the first place, Greece never came so completely into the power of a priestly class as did India; and in the second place, the practice on which it depended here came into rivalry with the already established practice of inhumation, which on the whole was cheaper. To this we should add the fact that the social institutions of the older race proved to be the more persistent, as with the Normans and Saxons in England, whence this must have been especially true of such ideas as we are discussing. And however spectacular and interesting the act of cremation became among the Hellenes, as reflected in the Homeric picture of the funerals of Patroclus and Hector, the accompanying conception of the soul after death could be but very vaguely imaged, as in the case of India; while the same idea accompanying burial in the ground, in cave-tombs, cist-tombs, and rock-tombs, as the so-called "treasury of Atreus" was capable of very definite imagery. Thus, although cremation continued to be practiced side by side with inhumation, it was the belief associated with the latter practice that possessed the more definite imaginative appeal, and that finally prevailed.

Yet the upper-world conception of the soul persisted and influenced the belief of later generations. As in the first instance it was only the Achæan masters of Hellas who practiced cremation, while the subject populace inhumed its dead; and since in the classical age it was only the wealthy who could afford cremation; so it came to be believed that the "good"—the worthy and the proud—at death went to heaven above, while the poor in purse and spirit descended into hell. Various modifications of this composite belief have grown up by internal suggestion and by accretions from foreign practices and beliefs; but in the last analysis each belief grew out of a practice, and the practice originated as an obvious and immediately practical necessity.

While we cannot say just where or why the Minoans developed inhumation and the Achæans cremation, or why some other practice did not arise and prevail among each people, yet it is perhaps significant that cremation was the practice of the northern race, like the aboriginal Hindus, —a people who had more need of fire on a large scale, such as would be necessary for the cremation of human bodies, a people with whom fire was necessarily a more continuous object of experience and therefore a more constant agent of purification in other ways also, than it was in the sunny southland of Crete and Hellas.

Homer was the poet of the Achæan overlords of Hellas. Yet he was apparently not of the Achæan race. Although he quite consistently presents to us the Achæan mode of burial, his idea of the soul and its abode is not consistent with the practice of cremation. He thinks of the cremated Heracles as having a corporate existence in Olympus, with lovely-ankled Hebe at his side; yet Heracles must also be seen of Odysseus in the house of Hades. Homer is himself aware of the contradiction, and declares it to be but a phantom that Odysseus sees there. On the other hand, Achæan heroes—Patroclus and others such as would naturally have been cremated—he unequivocally represents as being in the populous realm of Hades in the distant west. In Homer's references to the realm of the dead we discern the unconscious and inextricable mingling of at least three traditional views on the subject. Nor should we be surprised at this when we note that the entire period from the Trojan War to the final completion of the Homeric tales was one of ethnic amalgamation between at least the two races we have already mentioned. Our view of this process is still further complicated, and yet perhaps much illuminated, by the knowledge of a continuous intercourse with the west coast of Asia Minor during this time, such that most of the cities that laid claim to Homer were of this region.

And this prompts us to consider how the notion could have arisen that the dread abode of souls was in the west. It would perhaps be interesting to point to the west as the region of the setting sun, to associate it with the death of the day, and to conjure up some fancied analogy as having been indulged in by the aboriginal authors of this tradition. Yet in the face of such a procedure stands the fact that the west has always been the land of allurement and promise to which Greek no less than Teuton has ever turned his eyes. The fact is that if the association of the west is an essential element of the belief, as it appears to be, then thoughts of the west were inherently involved in the form of burial with which the belief was genetically associated.

We might look to cremation for the source of the association, if anywhere in the Ægean world the prevailing winds blew to the westward, thus bearing the smoke of the funeral pyre in that direction. But such is not the case; and besides, neither the earthly location of the Odyssean afterworld and the Islands of the Blest, nor the substantial, corporeal nature of the spirits dwelling there would permit of this conclusion.

I know not what may be the value of the suggestion I am about to make upon this subject; I simply present it as the most plausible explanation I can imagine for the conception of a western realm of the dead. I have by no means enumerated all the methods that man has employed for the disposal of his dead. Fundamentally there is but one reason for disposing of the dead by any means, and that is to secure a separation between the dead and the living. Inhumation and cremation are merely the most obvious and most universally practicable means of securing this one end.

Now one of the simplest modes of accomplishing this object, where natural facilities permit, is what is called canoe-burial,—a mode in which the body of the dead is placed upon a log, or raft, or boat, and set adrift upon the sea, or down a stream. In the course of time this practice, just as any other, is subject to elaboration and refinement, and finally to mythical, transcendental interpretation. I suggest that this Hellenic notion of a western realm of the dead originated on the western coast of Asia Minor. Here all rivers flow to the west; out to the westward over the sea are beautiful islands which could once have been imagined as the destination of bodies set adrift on the rivers of this coast; and finally, when these islands had been visited and explored and the fancy exploded, it was but natural to set the place of destination of the dead still farther to the west beyond the Ægean archipelago. And since even by Homer's time the Hellenes had dim fancies, more or less substantiated, of extensive coasts in the distant west, it was but natural that the earlier notion of an island abode for the dead had to give way to fancies of a more continental region. But as the primitive occupants of this Asiatic coast had grown bolder and put out to sea, they had perhaps found on the coasts of the Ægean islands the unsightly wrecks of their death-craft, and so had come to discontinue the practice. It is not necessary to suppose that this practice was current in the time of Homer, or even of the Trojan War; mythical fancies may survive long after the conditions that fathered them have ceased to exist.

Such is my suggestion for explaining the notion of a western abode of souls, presented on the assumption that both these traditions go back to a single local source. Yet I am not unmindful that the coast of Epirus and Illyria furnish the natural conditions in which either one or both may have arisen; whence we should have to suppose that they were brought into Greece by the Achæans. On this assumption we should have to suppose further that these Achæan adventurers, after leaving their native abode and the conditions supporting their native mortuary practice, took to cremation as a new means of disposing of their dead, and yet retained the tradition associated with the native practice of canoe burial. This would help to account for the incongruities in the Homeric conception of the condition of souls whose bodies had been burned; it would mean that they had not yet maintained the practice long enough to have invested it with a systematic sanction and philosophy. As between these two suggestions, I should probably prefer the former. As yet I see no way in which archeology may help us here.

In any case the tradition of a western abode of the dead, which had already been started and which had by this time lost all direct association with the practice, continued and gathered to itself the Homeric, and Hesiodic, and Pindaric refinements and differentiæ which we have already noted. Such is the regular course of tradition. It is undoubtedly in this way, and by reference to the same kind of burial practice in Britain that the traditional picture came to be built up of the black-hulled ship that bore "Elaine the fair, Elaine the beautiful" down the Thames to Westminster; and of that other dusky barge that bore out into the mystic lake beyond the ken of mortal man all that was mortal of good King Arthur. Such a social background is probably necessary for the historical interpretation of the death voyage of Sinfiotli, son of Sigmund, away "to the west"; and of Balder and his faithful wife Nanna, laid on their funeral pyre on the deck of the stately ship Ringhorn. We can understand and explain how a traditional practice arises and grows by social concurrence, and how a belief arises in association with it, all conscious association with the practice being gradually lost. But to explain how practice should arise out of an antecedent belief, and how that belief should first have arisen as a purely intellectual conception without sensuous motivation—as the grin without the cat, as one might say—in spite of some three thousand years of effort upon this problem, we are quite as far from a satisfactory solution as ever.

To conclude, then, the act of burial by early peoples is an act of aversion and riddance, even as the traditional interpreters of the act have claimed; but the primary object of the riddance, instead of being a metaphysical, or spiritual object, is a real, concrete, sensuous reality, which is exactly the necessary and apposite kind of motive that we should expect. If only Hobbes had hit upon this formula! But he had not at hand the rich accumulation of anthropological data that we now possess. And even Spencer and Tylor, with all the data at their command and with all their ability to analyze and organize their essential elements, made the same mistake as Hobbes. For in the first place they made belief about the dead a result of secondary sensuous experience, instead of primary; and secondly, they made it to depend upon visual instead of olfactory experience. The sense primarily concerned in the evolution of religious aversions associated with ideas of the dead is undoubtedly that of smell. This primary aversion, by a traditional transfiguration, becomes a dread or fear of the dead and places of burial; and only when man requests of his most-used sense to show him the cause of the aversion does it become visualized. And then only is it that dreams, visions, apparitions, reflections and other illusory visual phenomena gain a superstitious meaning.

Thus it is only by misinterpretation of the act of avoiding or allaying the noisome odors of decomposition, when the real motive to the act has disappeared from view, that a people can ever explain its burial practice as a spiritual "riddance" or "aversion," or as a "laying of the ghost." For the anthropologist to accept this secondary aspect of the relation between belief and practice as being primary, and to proceed upon this assumption to the explanation of burial practices is to put the cart before the horse. Such reasoning is all of a piece with myth; it is reasoning in a circle, and will never get us anywhere in the realm of scientific knowledge.

For such reasons as I have given above, which I believe to be sound, I feel reasonably certain that my primary assumption of an obvious and constant relation between the fact of death and beliefs about the dead is justified;

that geographical conditions have played a hitherto unrecognized part in the development of burial practice and belief about the dead; that the sense of smell has had an unrecognized share in the development of religious notions and especially religious fears; that the Greek notion of an underworld abode of the dead grew out of the practice of inhumation, and that the notion of a heavenly abode of souls in like manner grew out of the practice of cremation. And it is by reason of the satisfactory corroboration of my reasoning with regard to inhumation and cremation that I suggest a primitive practice of canoe-burial on the west coast of Asia Minor—or possibly the Balkan peninsula—as the primary motive to the conception of a western abode of souls, whether as Islands of the Blest or as a continental realm of dark-browed Hades.

ORLAND O. NORRIS.

YPSILANTI, MICHIGAN.

BERNARD BOLZANO.*

(1781-1848.)

IN BOLZANO we find the virtues of human sympathy and insight coupled with the austerer virtues of the metaphysician and logician. He was a man of action as well as a man of ideas. He was well known for his kindly disposition and his broadmindedness. He possessed not only the sympathy with the poor necessary for a social reformer, but the ability to develop his ideas of social reconstruction on practical lines. Not only did he elaborate a theory of an ideal state, but he also introduced numerous reforms in the actual state of which he was a member. He studied theology very earnestly as a young man and later wrote a great deal on the subject. Even though his liberal views brought him into collision with those on whom his livelihood depended, yet he courageously continued his teaching and writing, always making it his aim to seek for truth. He was a metaphysician of some importance and his treatises on metaphysics are valuable, not only for the original thought which they contain, but also for his important criticisms of Kant. In esthetics his work is by no means without interest, and to the psychology and ethics of his day he made very valuable contributions. But preeminently he was a mathematician and logician. In his work on mathematical analysis and mathematical logic, he stood out from all the other thinkers of his day. He was a man of many ideas and his intellectual equipment made him able to indicate to his followers the most fruitful lines of inquiry. All through his life he worked for the good of mankind, helping it on in its search for truth.

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* We regret that owing to limited time and the uncertainties of transatlantic mail service *The Monist* is compelled to go to press without receiving the author's *imprimatur*.

Bernard Bolzano was born on October 5, 1781, at Prague.[1] He was the fourth son of Bernard Bolzano, an upright and philanthropic member of the Italian community at Prague. His mother was a very pious women. He had a large number of brothers and sisters, the majority of whom perished in childhood; he himself was a sickly child. In his early youth he was very much interested in mathematics and philosophy. His education was of the type usual at the end of the eighteenth century. He tells us that as a child he used to let passion completely overmaster him because he believed that he was raging not at people but at Evil itself. Bolzano was sent to one of the gymnasia of his native city, where he did not distinguish himself very much, and later proceeded to the university there. At the university he studied philosophy and subsequently theology. It was his father's wish that he should be a business man, and though his father finally gave way he showed his disapproval of his son's desire to continue his studies in various ways.

Bolzano had been brought up a Roman Catholic and he was much troubled with doubts as to whether he should take orders. Finally, however, he became convinced that difficult problems, such as the authenticity of the miracles, were not essential parts of the Catholic faith, and as in his opinion the office of priest offered the best opportunity of doing good, he took orders in 1805. At the same time he became doctor of philosophy at Prague University, and was appointed professor of the philosophic theory of religion.

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[1] *Lebensbeschreibung des Dr. B. Bolzano mit einigen seiner ungedruckten Aufsätze und dem Bildnisse des Verfassers; eingeleitet und erläutert von dem Herausgeber* (J. M. Fesl), Sulzbach, 1836.

As professor, Bolzano suffered many cramping indignities which surrounded all teachers in Roman Catholic countries at that time. To a man with Bolzano's sympathies, the position must have been a peculiarly trying one. He had a great love for young people[2] and mixed freely with the students. He was particularly sought after by the students because of his liberal views. His broad-minded interpretation of the dogmas of the Catholic faith, while provoking the distrust of the authorities, recommended him to the younger generation, and he wielded a great influence in their revolutionary schemes and was thought by many to have supported them with an enthusiasm unbecoming in a professor. At any rate, relations between Bolzano and the authorities grew more and more strained, and finally, as he would not recall what they were pleased to call his "heresies," he was dismissed on the grounds that he had "failed grievously in his duties as priest, as preceptor of religion and of youth, and as a good citizen."

After his dismissal from Prague, two ecclesiastical commissions were successively appointed by the Archbishop of Prague to inquire into the orthodoxy of his teaching. In the first commission, the majority declared that Bolzano's teaching was entirely Catholic, but the word "entirely" was deleted at the wish of the minority—which consisted of one person. This decision so enraged the obscurantist party that a large amount of evidence (not a small amount of which was "faked" for the purpose) was collected and put before the second commission. In 1822 Bolzano made two declarations in writing in which he stated that he held it "dangerous, even with the best intentions, for a man to seek and teach new points of view as proofs of the truth and divine nature of the Christian Religion."[3] The commission then finally collapsed. Two years later Bolzano was pressed for a public recantation. The Archbishop of Prague brought illicit pressure to bear on him by pleading his affection for him and by declaring that a refusal would bring him to the grave. Bolzano, however, refused to recant publicly, but solemnly declared his orthodoxy in writing.

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[2] See A. Wishaupt, *Skizzen aus dem Leben Bolzanos: Beiträge zu seiner Biographie von dessen Arzte,* Leipsic, 1850, pp. 19ff.

The main points of his teaching on religion are set out at some length in his *Lehrbuch der Religionswissenschaft.*[4] He defines religion as the aggregate of doctrines which influence man's virtue and happiness. He then proceeds to discuss what seemed to him the most perfect religion, viz., the Catholic faith. His reason for so regarding the Catholic faith is that it is, in his opinion, revealed by God. A religion is divinely revealed, according to Bolzano, if it is morally beneficial and if connected with it there are supernatural events which have no other use than that they serve to demonstrate this religion. In the first chapter the concepts of religion in general, and organized religion in particular, are discussed. In the third chapter he maintains that for a religion to be true it must be revealed, and then he proceeds to enunciate the characteristics of a revelation. In the second volume, he sets out to prove that the Catholic religion possesses the highest moral usefulness and that its origin has the attestation of supernatural occurrences. He discusses the evidence for Christ's miracles and the genuineness of the sources and points out the presence in Christianity of the external characteristic of revelation. He then passes on, in the third volume, to demonstrate in some detail the moral usefulness of the faith. After a discussion of the Catholic doctrine of the sources of knowledge he examines the various doctrines of the

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[3] Published 1836 (Sulzbach) with autobiography.

[4] Sulzbach, 1839 (4 volumes).

Catholic church. It is interesting to notice that he regards the doctrine of the Trinity as entirely reasonable, and compares the Father to the All, the Son to humanity, and the Holy Ghost to the individual soul. In the last chapter of this volume Bolzano is concerned with the Catholic system of morals. In his investigation he discusses first Catholic ethics and then the various means of salvation recommended by the church. He examines each of the sacraments in turn.[5]

After his dismissal from Prague, Bolzano wrote a very great deal, but the internal censorship prohibited all publications in his name and even in some cases retained the manuscript. Bolzano once expressed the pious hope that some day he might be allowed to publish some work of a purely mathematical nature! After he left Prague he lived chiefly with friends at Techobuz. He came back, finally, to his native city in 1841 and continued his work with vigor until his death in 1848.

Though it was in mathematics that Bolzano did his most important work, yet in other subjects, notably in political science, his work is of considerable value. He had very great sympathy with the poor and was anxious to abolish class differences. He was convinced that the inadequacy of social organizations was the cause of poverty. He never wrote very much on the matter, but made it the subject of many of his professorial addresses. There is, however, one short manuscript[6] in which he sets out the main points of his political theory. Bolzano himself thought a great deal of this manuscript for he says in the introduction: "And small as is the number of these pages, yet the author thinks he may be allowed to attribute some value to them. Nay, he considers that this little book is the best and most important legacy that he can bequeath to his fellow men if they are willing to accept it."

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[5] For a complete list of his theological works see Bergmann, *Das philosophische Werk Bernard Bolzanos,* Halle, 1909, p. 214.

[6] "Vom besten Staate, MS. in the Royal Bohemian Museum. For a convenient summary of the MS. see Bergmann, *op. cit.,* pp. 130ff.

In Bolzano's ideal state, men and women alike are to have the privilege of voting, but a person is only allowed to vote on a matter of which he has some knowledge and in which he has some interest. Further, the right of voting is liable to forfeiture in the case of misconduct. Any citizen may put forward a suggestion. The suggestion is examined by six independent citizens, each one examining it privately, and it is only rejected if all six of the citizens reject it—and even then it is retained by the state for further reference. If it is not rejected, a general vote is taken, and if there is a majority in favor of it, it goes to a council[7] which is composed of men and women over sixty years of age, who are chosen by the people every three years. The council can only veto the decisions of the people if ninety percent of the council are against it. The government is the administrative body, its members are paid and elected by the people, and there is a strict limit to the length of time that they may remain in office. The government takes special care to prevent private individuals combining in their own interest. Bolzano looked upon war as a dreadful misfortune and in his Utopia war is only to be used as a defensive measure. Bolzano points out that internal revolutions are unlikely, for they arise in general from one of two causes—a bad constitution or poverty. Of these, poverty is to be non-existent and a revolution due to the first cause is improbable because it could only be brought about if the council opposed a change in the constitution which the people considered advisable. But the council in its wisdom would not taunt the people but would give reasons for its decision. It therefore seems unlikely that the people would rise in revolt, all the more because it is early impressed upon the young that a good citizen does not work against the government, for the government's object is to work for the good of the whole state.

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[7] The council is called the "*Rat der Geprüften.*"

One of the most interesting parts of the manuscript deals with the idea of property. In the ideal state property is only desired in so far as the possession of it contributes to the common good. The only valid claim of a man to property is, therefore, that he can make it more useful to the state than any one else could. The fact that a man may possess a certain thing at a certain time is not a necessary or sufficient reason that he shall possess it altogether. The right of inheritance is not recognized. Things such as books, paintings, furniture or jewels, are given to a citizen to use but not to possess. Further, even though he may have established his claim to a certain object, yet, if at any subsequent time another citizen can make more use of it, the title of the first citizen to it is gone. Moreover, the state does not offer any compensation to a man for depriving him of anything. Thus a man whose eyesight has been cured has his glasses taken away and no compensation is made. In all the distribution of goods the government is guided entirely by the principle that the use of a certain thing should be granted to the citizen who can render it most useful to the state as a whole.

The ideals of the state are freedom and equality. There is no unequal distribution of wealth. However there is not an absolute equality of owners, for, as Bolzano points out, the possibility of increasing one's property is a powerful incitement to work. But there are limits beyond which a man cannot increase the extent of his property, and these limits are determined by the consideration of the good of the state as a while. There are "equal" right for all citizens, but the word "equal" is not to be interpreted in any narrow sense. Rather there is an adjustment between the rights of a citizen and his obligations, between his strength of pleasant sensations. Not the least interesting part of his work in ethics is his criticism of Kant's categorical imperative. He urges the necessity for a modification in Kant's principle and points out the invalidity of Kant's theory that the opposite of a duty involves a contradiction.

Bolzano's work in esthetics is not without interest.[9] His theory of esthetics is the result, not of his own esthetic sensations, but of a painstaking analysis of the abstract idea. His definition of the scope of the subject does not make it coincide with the theory of beauty unless we include in that theory not only the sum total of truths directly concerned with beauty but also all those which stand in such a relation to them that either the former cannot be thoroughly understood without the latter or the latter without the former. To get at his concept of beauty, he eliminates goodness and attractiveness, and by this process obtains a first criterion of beauty, viz., all beauty is pleasant, i. e., it produces pleasure and this pleasure arises solely from the contemplation of the object. Further, since animals are to be excluded from esthetic enjoyment, qualities must be introduced which they do not possess, e. g., intelligence, judgment and reason. Bolzano then comes to the conclusion that it is the growth of these qualities in us that is responsible for the pleasure we find in beauty. Together with the "Ueber den Begriff des Schönen" in the Royal Bohemian Museum, there is another short treatise of Bolzano's in which a theory of laughter is elaborated.[10] Bolzano thought that laughter was caused by the rapid alternation of pleasant and unpleasant sensations and from the fact that animals and infants do not laugh he deduces that laughter is not entirely physical.[11]

In his metaphysics, Bolzano reveals himself as "one of the acutest critics of the Kantian philosophy and the 'idealist' development from Fichte to Hegel."[12] He also did some important original work. His chief book on the subject,[13] entitled *Wissenschaftslehre: Versuch einer ausführlichen und grösstenteils neuen Darstellung der Logik,*[14] is divided into five sections. In the first of these he sets out to prove that objective truth exists and that it is possible for us to have knowledge of it; but he allows that in the development of the science of knowledge, which is the most fundamental of the sciences, it is necessary to use some psychological methods of treatment. In the second part, the "Theory of Elements," he treats successively ideas-in-themselves, their combination into propositions-in-themselves, the theory of true propositions-in-themselves, and finally their combination into syllogisms. He is extremely careful to distinguish between the idea-in-itself and the conceived idea. The concept of a proposition-in-itself is produced by a double abstraction. First the meaning of the proposition and the words conveying the meaning have to be separated from each other, and then one has to forget that the proposition has ever been in anybody's mind. By this means we get to the concept of a proposition-in-itself.

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[9] See *Ueber den Begriff des Schönen*, Prague, 1843.

[10] *Ueber den Begriff des Lächerlichen*, 1818.

[11] See Bergmann, *op. cit.*, Part IV, § 56.

In the distinction that he draws between perception and conception, Bolzano himself says that he owes very much to Kant, but Bolzano disagrees with him in the use he makes of this distinction in his theory of time and space. Bolzano examines in some detail Kant's theory of time and space and his theory of the categories, making some very acute criticisms. After an investigation into the theory of the syllogism and a discussion of the function of the linguistic expression of a proposition, the "Theory of Elements" closes with a criticism of previous works on the subject. Next Bolzano considers the appearance in the mind of propositions-in-themselves. And it is in this part of his work in particular that we see the extent and depth of his learning. He treats first our subjective ideas, then our judgments, then the relation of our judgments to truth, and finally their certainty and probability. In this investigation Bolzano uses psychological methods to some extent. Then after the fourth part, the "Art of Inventing," he comes at last in the fifth part to the "Science of Knowledge Proper." The book is remarkable as much for its wealth of original thought and the clearness of expression as for the important criticisms of earlier works on the subject.

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[12] A. E. Taylor, *Mind,* October, 1915.

[13] For a criticism of Bolzano's theories see M. Palagyi, *Kant und Bolzano,* Halle, 1905.

[14] Sulzbach, 1837.

But important as is Bolzano's work in metaphysics, ethics, esthetics, and theology, it is preeminently as a mathematician that he should be remembered. Now there are two ways of looking at mathematics. One can look upon it as Huxley did: "Mathematics may be compared to a mill of exquisite workmanship, which grinds you stuff to any degree of fineness." On the other hand, one can look upon mathematics as a real and genuine science and then the applications are only interesting in so far as they contain and suggest problems in pure mathematics. From the second point of view the most important business of the mathematician is to examine and strengthen the foundations of mathematics and to purify its methods. In addition to these points of view which may be called the practical and the philosophical, a third point of view has sprung up in the last century which may be called the purely logical point of view. Whitehead describes this new point of view in the words, "Mathematics in its widest significance is the development of all types of formal, necessary, deductive, reasoning."[15] In this purely logical system, it is proposed to treat any special development of mathematics with the help of a definite, logically connected complex of ideas, and the mathematician is not to be satisfied to solve particular problems with the help of any methods which may casually present themselves, however ingenious these methods may be. Clear definitions and unambiguous axioms must be framed and then by rigorous reasoning the theorems of the subject are to be deduced.

We find examples of the first and second points of view among the Greeks. It is said of Pythagoras that "he changed the occupation with this branch of knowledge into a real science, inasmuch as he contemplated its foundation from a higher point of view and investigated the theorems less materially and more intellectually,"[16] and of Plato that "he filled his writings with mathematical discussions, showing everywhere how much geometry there is in philosophy." Just as mathematics among the Greeks had its origin in the geometry invented by the Egyptians for practical surveying purposes, so the mathematics of the seventeenth and eighteenth century received its stimulus from the practical researches of Kepler, Newton and Laplace. But in this same fragment of Eudemus we find it recorded that Euclid tried to revise the methods used and "put together the elements, arranging much for Eudemus, finishing much for Thaetetus; he moreover subjected to rigorous proofs what had been negligently demonstrated by his predecessors."

This same work that Euclid did for Greek mathematics three hundred years B. C., the new school of nineteenth century mathematicians performed for European mathematics. The researches of Newton had suggested a wealth of material for mathematical treatment. Newton and Leibniz had stumbled across the powerful methods of the calculus, which were of tremendous practical importance; but as Klein says, "the naive intuition was especially active during the period of the genesis of the calculus,"[17] and in the great call for powerful methods the theoretical side was almost entirely overshadowed. For example Newton assumed the existence of the velocity of a moving point at every point of its path, not troubling whether, as subsequent investigation has shown to be the case, there might not be continuous functions having no derivative. The great work then of this new school was to investigate the validity of the methods used in the two previous centuries. This was no easy task, and it is only now after one hundred years that the theory of the subject is being put on a logically satisfactory basis. The most important ideas round which the greater part of the work in mathematics centered, are those of continuity and infinity. The importance of these concepts became apparent from the work done on infinite series. A particularly simple example of series, viz., decimal fractions, was in use as early as the sixteenth century, but Leibniz was the first mathematician to have any idea of the importance of series in mathematics. Before his time it had not been realized that an infinite series can only have a meaning under certain circumstances. Unfortunately Leibniz came to the conclusion that the sum of the series

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[15] A. N. Whitehead, *A Treatise on Universal Algebra*, Cambridge, 1898, preface, p. vi.

[16] Extract from a fragment preserved by Proclus; generally attributed to Eudemus of Rhodes who belongs to the peripatetic school and wrote treatises on geometry and astronomy. See extracts in J. T. Merz, *History of European Thought in the Nineteenth Century*, Vol. II, p. 634.

$$1 - 1 + 1 - 1 \ldots . \textit{ad inf.}$$

is $\frac{1}{2}$,[18] and so exercised a somewhat baneful influence on subsequent mathematical developments of the theory of infinite series. However it was left to the genius of Bolzano[19] to enunciate for the first time the necessary and sufficient conditions for the convergence of an infinite series. In 1804 Bolzano published his *Betrachtungen über einige Gegenstände der Elementargeometrie* (Prague), and in 1810 his *Beyträge zu einer begründeteren Darstellung der Mathematik* (Prague). In 1816 he published an important tract on the binomial theorem. In this tract his work on convergency is of great value and his investigation for a real argument (which he everywhere presupposes) is very satisfactory. Bolzano comments on the unrestricted use of infinite series which was common at the time. In 1812 Gauss had published an investigation into the circumstances under which the hypergeometric series converges, and in 1820 Cauchy delivered some extremely important lectures on analysis at the Collège de France, where he was the leader of a group of young mathematicians. Thus Bolzano, Gauss and Cauchy were the pioneers. In his book, *Der binomische Lehrsatz und als Folgerung aus ihm der polynomische und die Reihen, die zur Berechnung der Logarithmen und Exponentialgrössen dienen, genauer als bisher erweisen* (Prague), Bolzano has made a valuable criticism of earlier investigations. It is remarkable that his writings, though of great importance, received comparatively little attention at the time. According to Merz, he had not, like Cauchy, "the art peculiar to the French of refining their ideas and communicating them in

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[17] *Evanston Colloquium; Lectures on Mathematics delivered September,* 1893, Lecture VI.

[18] Euler in 1755 (*Instit. Calc. Diff.*) defined the sum of this series to be $\frac{1}{2}$. In the recent theory of divergent series (due in great measure to E. Borel see his *Leçons sur les séries divergentes,* Paris, 1901) one way of defining the formal sum of a divergent series Σa_n is as the limit, when it exists, of $\Sigma a_n x^n$ as x tends to unity through values less than unity. This definition has the

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merit of simplicity and also of "consistency," i. e., When the series Σa_n converges, its sum is still the limit as x tends to unity through smaller values, of $\Sigma a_n x^n$ if this limit exists.

Defining the formal sum in this way the sum of the series $1 - 1 + 1 \ldots$ *ad* inf. is ½.

[19] Accounts of Bolzano's mathematical work were given by Otto Stolz (*Math. Ann.*, Vol. XVIII, 1881, pp. 255-279; Vol. XXII, 1883, pp. 518-519) and on pp. 37-39 of the notes at the end of the reprint of Bolzano's "Rein analytischer Beweis" of 1817 in No. 153 of *Ostwald's Klassiker*.

the most appropriate and taking manner."[20] In his *Rein analytischer Beweis* (1817) Bolzano tells us that it is very much better to publish one's mathematical work in separate treatises; in this way there is more chance of getting acute criticism. Consequently we find his mathematical work scattered about in various small treatises.[21] Also he tells us that one of his treatises had the misfortune not to be noticed by some of the learned periodicals and in others to be criticized only superficially.

In 1842, in the course of some work on the undulatory theory of light, he made a prophecy which is extremely interesting in the light of the invention of spectrum analysis and the researches of Sir W. Huggins, Kirchhoff, and others. He said: "I foresee with confidence that use will hereafter be made of it in order to solve, by observing the changes which the color of stars undergoes in time, the questions as to whether they move, with what velocity they move, how distant they are from us and much else besides." But let us return to the most *i*mportant part of Bolzano's mathematical investigations.

In 1817 Bolzano published a paper we have already mentioned entitled "Rein analytischer Beweis des Lehrsatzes: dass zwischen je zwei Werthen, die ein entgegengesetztes Resultat gewähren, wenigstens eine reele Wurzel der Gleichung liege." This paper is, in a way, his most important work and is a triumph of careful and subtle mathematical analysis. His central theorem, as the title indicates, is as follows: If in an equation $f(x) = 0$, $x = \alpha$ makes $f(x)$ positive and $x = \beta$ makes $f(x)$ negative, then there is at least one real root of the equation $f(x) = 0$ between α and β. Before he begins his constructive work he criticizes very acutely the previous attempts of Lagrange and others. He points out the errors that had been made by previous investigators and he emphasizes once more the great importance of freeing mathematical analysis from the intuitional treatment to which it had formerly been subjected. In order to prove his main theorem, Bolzano found it necessary to introduce the concept of the continuity of a function, the notion of the upper limit of a variate and some important work on infinite series. His method is briefly as follows:

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[20] *Op. cit.*, Vol. II, p. 709.

[21] For complete list see Bergmann, *op. cit.*, pp. 213-214.

1. He introduces the concept of "continuity." A function is said to be "continuous" for the value x if the difference between $f(x+\omega)$ and $f(x)$ can be made less than any assigned number, however small, if only ω is taken sufficiently small.

2. He discusses the convergence of infinite series and makes the following important statement. "If the difference between the value of the sum of the first n terms and the first $n+r$ terms of a series can be made as small as we please, for all values of r, if only we take n large enough, then there is one number X and only one such that the sum of the first p terms approaches ever more and more nearly to X as p increases." Unfortunately his proof of this theorem is not rigorous and his discussion only renders the existence of X probable.

3. From his work on infinite series Bolzano passes on to an extremely important theorem in which he introduces the new idea of an upper limit. And the theorem, as it occurs in this paper, gains in importance from the fact that the method used is one of fundamental importance in analysis. The theorem runs as follows: "If u_n be such a number that the property M holds for all values of x which are less than u_n, and if the property does not hold for all values of x without exception, then of all the numbers u_n satisfying this condition there is one (say U) which is greater than all the others." This theorem, which might appear obvious to those who allow their geometrical in-

tuitions to cloud their mathematical ideas, is proved by Bolzano with great care and completeness. The method used in the proof was used a great deal by Weierstrass and is now known as the "Bolzano-Weierstrass" process. As the method is of such great importance, we will indicate roughly the way it is used in the proof of this theorem. It will be convenient to call x's which have the property M "suitable" x's and x's which do not have the property M "unsuitable" x's; and further to call a number N a "suitable" number if all x's which are less than N have the property M, and to call a number N an "unsuitable" number if there are some values of x, less than N, which do not have the property M. Now it is obvious that there is a positive number D, such that $u_n + D$ is an unsuitable number. Then, bisecting the interval between u_n and $u_n + D$, we get the number $u_n + D/2$; bisecting the interval between u_n and $u_n + D/2$ the number $u_n + D/2^2$; and so on. When either all the numbers $u_n + D/2^r$ for $r = 1, 2, 3 \ldots$ are unsuitable or there is a number R such that $u_n + D/2^R$ is an unsuitable and $u_n + D/2^{R-1}$ a suitable number. In the first case the existence of U is established, U being equal to u_n. In the second case we repeat the process, dividing the interval between $u_n + D/2^{R-1}$ and $u_n + D/2^R$. Again, either all the numbers $u_n + D/2^R + D/2^{R+s}$, $s = 1, 2 \ldots$ are unsuitable or there is a number S such that $u_n + D/2^R + D/2^{R-S}$ is an unsuitable and $u_n + D/2^R + D/2^{R+S} - 1$ a suitable number. We continue the same process: if it does not terminate we get finally to an infinite series

$$u_n + D/2^R + D/2^S + D/2^T + \ldots$$

and since R, S, T... are positive integers the series obviously satisfies the conditions of the theorem in paragraph (2) above, and so there is a definite limit to which it tends.

this limit being the "upper limit" U in question. The existence-theorem for an upper limit is thus established.

4. Bolzano next attacks the following theorem: "$f(x)$ and $\varphi(x)$ are continuous functions of x and for $x=\alpha$, $f(x)<\varphi(x)$ and for $x=\beta$, $f(x)>\varphi(x)$: then there is a value of x between α and β for which $f(x)=\varphi(x)$." We will indicate the method Bolzano uses to prove it and we shall see exactly why he found it necessary to establish the existence of an "upper limit." Bolzano shows that, since $f(x)$ and $\varphi(x)$ are continuous, there is a number ω such that all numbers less than it satisfy the relation $\varphi(\alpha+\omega)>f(\alpha+\omega)$. Such a number we may call as in paragraph (3) a "suitable" number. Then from a direct application of the theorem about an upper limit he establishes the existence of an upper limit, say U, for all suitable numbers. It is then easy to show that $f(\alpha+\mathrm{U})$ cannot be less than $\varphi(\alpha+\mathrm{U})$ and cannot be greater than $\varphi(\alpha+\mathrm{U})$ and is therefore equal to $\varphi(\alpha+\mathrm{U})$. In this kind of way Bolzano proves the existence of the value of x between α and β giving $f(x)=\varphi(x)$.

5. Finally Bolzano proves that an expression of the form

$$a+bx^m+cx^n+\ldots+px^r,$$

in which $m, n, \ldots r$ are positive integers, is continuous. Then by means of an easy application of a slightly modified form of the theorem in (4) he proves that there is at least one real root between α and β. The whole paper is extremely valuable and it is interesting to see how Bolzano was led from his central theorem to the theorem in (4), to the concept of "continuity" and the idea of an "upper limit," and in the existence-theorem for the upper limit to the question of the convergence of series.

In mathematical logic and in the theory of infinite numbers, Bolzano's work was also of great importance. Bolzano's definition of the continuum is of some interest in itself. He defines a continuum as a set of points such that every point has another point also belonging to the set as near to it as we please.[22] This is expressed in modern phraseology by saying that the continuum is a set of points which is "everywhere dense." The name continuum is now used (after Cantor) only for a set of points which is not only "everywhere dense" but also "perfect." A set of points is "perfect" when every convergent sequence has a limit which is itself a number belonging to the set, and conversely when every number is the limit of properly chosen convergent sequences of numbers themselves belonging to the set.[23] Thus Bolzano would call the set of rational numbers a "continuum," but this set is not perfect and is therefore not a "continuum" in the modern sense of the word. In his work on infinite numbers Bolzano anticipated to some extent the work of Georg Cantor. An "infinite" collection is defined to be a collection which has no last term.[24] He proves that the number of natural numbers and the number of real numbers is infinite, and he sees (§ 49) that the number of these two collections is different. Bolzano also recognizes the fact that it is possible to arrange the points in two lines of different lengths so that each point of one collection corresponds to one single point of the other collection and *vice versa,* no point being left without a corresponding point. This brilliant idea of a one-one correspondence went a long way toward dispersing the cloud of mystery which hung over the contemporary infinite number. Leibniz had stated the difficulty quite plainly. Every number can be doubled, he said, therefore the number of natural numbers and the number of even natural numbers is the same. Therefore the whole is equal to the part—which is absurd. Bolzano realized that there is no real contradiction in this. This same idea of the one-one correspondence between points belonging to certain sets of points has led to the modern idea of "reflexiveness" of infinite numbers. The property of "reflexiveness"[25] together with that of "non-inductiveness,"[26] which disposes of all attempts to count up infinite collections or identify the number of terms in an infinite collection with the ordinal number of the last, has removed all serious difficulties and has helped to make it possible to put the concept of an infinite number on a logical foundation.[27] Defining "similar" classes as classes whose terms have a one-one relation to each other and the "cardinal number" or "power" of a class as the class of all similar classes, we see immediately that the class of natural numbers and the class of even natural numbers have the same cardinal numbers. Thus Bolzano was quite right in seeing no contradiction in Leibniz's statements.

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[22] *Paradoxien des Unendlichen,* Leipsic, 1851, 2d ed., Berlin, 1889, § 38.

[23] See E. W. Hobson, *The Theory of Functions of a Real Variable and the Theory of Fourier's Series,* Cambridge, 1907, p. 49.

[24] *Paradoxien des Unendlichen,* § 9.

From these few references to isolated theorems and statements in Bolzano's work, it is seen that he had most of the ideas essential in the modern view of mathematics, and that in mathematics at least Bolzano's work has been a source of inspiration to those who came after him. Whether in his theology, his ethics, his political science, his metaphysics, or his mathematics, the desire for clearness of concepts was always his aim. Even the parts of his work which are no longer of intrinsic interest, e. g., his esthetics or his theory of laughter, have an interest for us in that they show us the methods he used in seeking for truth. That there is objective truth and that we can have knowledge of it—this was the thesis which he set before him in his work. In mathematics especially his work was needed, for whereas idealists maintained that mathematics deals only with appearances, empiricists insisted that mathematics could only approximate to the truth. Bolzano's life work was to start mathematicians on the right way to refute both the idealists and the empiricists. His method of strictly logical analysis of the ideas of continuity and the infinite was the clue which was followed up by all the great mathematical logicians and mathematical analysts of the nineteenth century, until finally the fundamental thesis has been proved that all concepts of pure mathematics are wholly logical. Thus Bolzano was one of the first to suspect and in this he was a worthy successor of the great Leibniz. Unlike most mathematicians of his day, Bolzano did not in his thirst for results succumb to D'Alembert's maxim, *Allez en avant, la foi vous viendra.*

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[25] A number is said to be "reflexive" if it is not increased by adding one to it. See B. Russell, *Our Knowledge of the External World as a Field for Scientific Method in Philosophy*, Chicago and London, 1914, p. 190.

[26] A number is said to be "non-inductive" if it does not possess deductive properties. See B. Russell, *op. cit.*, p. 195.

[27] Cf. the definitions "that which cannot be reached by mathematical induction starting from 1" and "that which has parts which have the same number of terms as itself," B. Russell, *The Principles of Mathematics*, Cambridge, 1903, Vol. I, p. 368.

We live in days when some of the contradictions and paradoxes which have perplexed the human race since the days of Zeno are being finally cleared up. Do not let us forget the work of Bolzano who with painstaking endeavor sowed the seeds of this great revolution in mathematical ideas.

DOROTHY MAUD WRINCH.

CAMBRIDGE, ENGLAND.

A MEDIEVAL INTERNATIONALIST.

ARBITRATION, a league of peace and a council of conciliation seem to be very modern suggestions as methods of avoiding war between civilized nations. Some hints of these, however, can be found in Kant's *Perpetual Peace* and in the *grand dessein* as expounded by the Abbé de S. Pierre. These schemes belong to the Revolutionary and Renaissance periods. But even before, in the Middle Ages, similar schemes are to be found in the work of Petrus de Bosco (Pierre Dubois).

The political acuteness of this brilliant thinker can only be understood by allowing for the fact that he had listened at Paris to "that most prudent friar Thomas Aquinas"[1] and by remembering that he wrote while the official politicians were engineering war after war for no purpose. His work on international politics is contained in the unprinted *Summaria brevis.....abbreviationis guerrarum* and in the *"De recuperatione Terre Sancte,"* published (1891) in the *Collection des Textes.* I propose to summarize and comment upon the latter, not as of merely archeological interest, but as an early attempt to grapple with the same political problem which we now face.

The treatise is supposed to deal with a plan for recovering the Holy Land and is addressed in 1306 to Edward I, "King of England and Scotland, Lord of Ireland and Duke of Aquitaine," as a great legislator and one who was specially interested in a new crusade. But this initial purpose of the treatise, even if it was intended by the author as more than a mere *captatio benevolentiae,* is certainly subordinated to the general problem of international policy among the European states.[2] The order of the argument is confused, the author continually going back to a subject after he has left it for some other. He writes well, but too eagerly to be as exact as the philosophers of his day. He is genuinely excited by the pressing importance of establishing peace. I shall, therefore, not follow the order of the treatise, but state first the nature of the problem as it appears to Dubois and then his suggestions for solution.

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[1] Par. 63, *De recup. Terre Sancte.* (In medieval Latin final *æ* becomes *e.*)

War between European countries and kings, says Dubois, is the chief hindrance to "having time for progress in morality and knowledge." War breeds war until war becomes a habit.[3] The deaths of one war cause speedy preparations for revenge.[4] "We should seek a general peace and pray God for it, that by peace and in time of peace we may progress in morality and the sciences, since we cannot otherwise; as the Apostle feels when he says: 'The peace of God which passeth all understanding keep your hearts and your minds:' your minds, which are reasonable souls, are not kept but are often destroyed by wars, discords and civil brawls which are like wars, and by the continuance of all such. Therefore, as far as he can, every good man should avoid and flee them; and when he takes to war, being unable otherwise to obtain his rights, he ought as much as possible to shorten it. . . . Thus universal peace is the end we seek."[5]

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[2] Guillaume de Nogaret uses the same pious cover for his scheme of social reform. One had to bow, so to speak, to the crusading ideal and then one was free to suggest anything!

[3] Quanto frequentius bella committunt, tanto magis appetunt committere, hoc consuetudine magis quam emendatione deputantes." Par. 2.

[4] "Ad bellum et vindictam voluntariam se preparant."

[5] Par. 27, *in fine.*

It is agreed that peace is desirable; but, says Dubois, "since it is proved that neither the Scriptures, nor sermons drawn from the Scriptures, nor the elegant lamentations and exhortations of preachers have been sufficient to stop frequent wars and the temporal and eternal death of so many human beings which have resulted, why should there not be found at last a new remedy for militarism (*remedium manus militaris*), as for example a judiciary backed by force (*justicia necessario compulsiva*)?" (par. 109). "This is an argument," he declares, "to which a reply is impossible morally and politically speaking." Peace has come within states by *vis coactiva*: so also it will come between states. One could not have a clearer statement of political judgment upon the evidence. The author himself says that he depends upon experience for his opinions: and he declares that exhortations to peace and praise of its excellencies and even rhetorical attacks on war are politically valueless. They have been tried and they have failed.

Before speaking, however, of the means by which peace is to be established between states, we must notice the plan which is *not* suggested by Pierre Dubois. The governing ideal of medieval politics, unity, led many to look for peace through subordination to one overlord. "Now there is no sane man, I think," Dubois writes, (par. 63), "who could think it likely that in this latest age (*in hoc fine saeculorum*) there could be one monarch of the whole world in temporal affairs who would rule all and whom as superior all would obey. For, if there were any attempt at this there would be wars, seditions and discords without end; nor would there be any one who could allay them by reason of the number of different nations, the distance and distinction between countries and the natural inclination of men to diverge. Although some have been popularly called "lords of the world" nevertheless I think that since the countries were settled there never has been any one whom all obeyed." That passage, if it seems to condemn Dante as a *homo non sane mentis,* certainly shows an historical acumen and a political judgment far superior to the opinions of the *De Monarchia.* Dubois recognizes the impossibility of arriving at peace by means of the conquest by one state of all other states. He sees that world-power is nonsense.

It must be admitted, however, that from the passages of the *Summaria brevis* which have been commented upon by M. de Wailly and Ernest Renan, one might judge that Dubois hoped for a domination in Europe of the French king. He held, indeed, that it should be arrived at by diplomacy and not by war, but in the above passage of the *De recuperatione* he seems to condemn not merely any special means, but dreams of domination by a single lord.

Inconsistency may be urged against him, and yet it must be remembered that here he is writing to the English king and also that he may very well have felt uncertain as to how the *vis coactiva* above the warring states might be established, even if he held quite clearly to the notion that the ultimate supremacy of one monarch was impossible. But let us turn to the definite political means he suggests for establishing peace between European states.

The means by which such peace is to be arrived at are: *First*: International arbitration and the establishment of an international judiciary. This is to begin by a general council (par. 3), a preliminary to all medieval and early Renaissance plans for reform. But what is unusual in Pierre Dubois is the statement that the difficulty of arranging matters is due to the fact that the cities of Italy, for example, and the various princes acknowledge no superior. "Before whom then," he asks, "can they bring their disputes? It can be answered that the council should establish elected arbiters (*arbitros*) religious or others, prudent, experienced and trustworthy men." These are to select three prelates and three others for either party to the dispute. They are to be well paid and such as are not likely to be corrupted by affection, hate, fear, greed or otherwise. They are to meet at a suitable place, to have presented to them in a summary and clear form, without minor and unimportant details, the pleas of either side. They are to take evidence from witnesses and documents, each witness being examined by at least two trustworthy and careful members of the "jury." The depositions are to be written and preserved. "For the decision, if it is expedient, they are to have assessors (*assessores*) who are thought by them most trustworthy and best trained in the divine, the canon and the civil law."[6]

Secondly, these decisions must be made effective. The Holy See is recognized as an influnce, but excommunication had better not be used. "Temporal punishment, although incomparably less than eternal, will be more feared."[7] The suggestions in detail of Pierre Dubois are perhaps a little comic, but we must allow for the situation. In the first place any group making war shall, after the war is over, be removed bodily and sent to colonize the Holy Land! If they do not oppose the movement, they may take some of their property with them. The author feels that it may be difficult. He then goes on as to other measures. Suppose, he says, that the Duke of Burgundy declares war against the King of France,—the king should then institute an economic boycott[8] and by a general council the same boycott should be declared by all Europe. Active military measures should be taken to devastate the country so that the whole people should feel it: Dubois, it seems, would adopt extreme measures to prevent war spreading, his main

[6] Par. 12, *De recup. Terre Sancte.*

[7] Excommunication is to be used (§ 101) but not depended upon by itself. Any one refusing to enter the league of peace (*pacis universalis federa*) is to *be immediately attacked.*

[8] Prohibebit quod nullus ad terras eorum deferat victualia, arma, merces et alia quaecumque bona, etiam quacumque causa sibi debita," (par. 5).

point being that in whatever corner it broke out the whole of Europe should act together and at once to stop it.

The reader may feel that this is hopelessly unpractical, since we could not act thus against any great country or against any combination of countries. But we must remember (1) that Dubois supposes Europe to be one political system (*respublica Christicolarum*) able to act in concert at least in some issues, and (2) that every war begins, according to him, in some comparatively small group. Thus practically, if Europe had adopted strong economic, even without military, action during the Balkan wars of 1912 and 1913, the war of 1914 might never have occurred. And surely it is not unpractical to suggest that all civilized countries should act together in the case of any conflict breaking out such as that of 1912. Deal effectively with the small conflicts and the first difficulty is met with regard to the larger. But one can imagine the horror of medieval diplomatists if all the states were asked to prevent any small wars by direct intervention of enforcing arbitration. Even to-day all the schemes for rearranging international politics start from the present almost universal war. I cannot help feeling, however, that Dubois was right. Our schemes for doing without war must inculcate combined action in *small* wars. Deal effectively with them and we may never have to deal at all with war between great states. It is the spark, not the conflagration, that we must consider first: and perhaps European diplomacy was more futile in 1912 than in July 1914, although the results of inaction did not show themselves till August, 1914. But let us return to the general thesis and omit further applications of it.

After details as to raising funds for a common force and plans for a common advance on the Holy Land, Dubois recalls himself to his main interest, "a general peace." In the *third* place therefore, he says that no external measures will be effective until the religious attitude is changed.

This opens an elaborate project for the reform of the Roman church. Dubois says (par. 29) if the pope really wants to stop war "he must begin with his brothers the cardinals and his fellow bishops." They are always going to war (*ipsi guerras movent*). Their attitude is quarrelsome even in England and France where they do not actually fight. The monks are as bad. But the whole attack is common to many writers of the date of Pierre Dubois. His remedies are extreme. First he suggests that if the pope had no "temporal power," no one need to go to war for him and that would be a beginning; and next, he actually proposes the confiscation of ecclesiastical property by states and the use of the wealth for common European civilization![9] But how?

The *fourth* suggestion of Pierre Dubois is that the money should be spent in education.[10] The purpose of the education, according to the general thesis as to the taking of the Holy Land, is directed by the general need of non-military contact with the East. It is urged that you can only hold the East effectively by intellectual superiority to it.

Then begins a long and elaborate scheme of education, primary and secondary. University education is implied but not dealt with in detail. All this is to occur in the Holy Land. It is a well-known medieval trick for writing a Utopia. In 1223 "The Complaint of Jerusalem" gave a plan for reconstructing European society under the guise of a scheme for an Eastern kingdom. So here Dubois, appearing to speak of what ought to be done when the Holy Land is established as a state, is really speaking of the remedies which ought to be applied in Europe. In the matter of education he is as original as in politics, but what is most interesting to us now are the hints for bringing the European nations together. Colleges for boys and for girls are to be established where "modern languages" are to be taught—"the literary idioms, especially of Europe, that by these scholars trained to speak and write the languages of all, the Roman church and the princes of Europe should be able to communicate with all men." Some are also to be taught medicine, some surgery—the girls also (par. 61); and these girls, in the medieval fashion perhaps, are to be married to foreigners, even Orientals (*ditioribus Orientalibus in uxores dari*). I need not detail the plans for intermarriage and colonization, among which is inserted a suggestion for a married clergy (par. 102). A long section follows upon the utility of scientific knowledge "according to brother Roger Bacon" (par. 79) and upon the variety of human knowledge in general. There are interesting hints as to the transformation of convents into girls' schools, and as to military reform, for example the institution of definite uniforms (par. 16). But all these do not bear directly upon his plans for peace and we may therefore omit them here. His boldness of conception is clear.

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[9] Par. 57. "Que tendit ad reformationem et unitatem veram totius reipublice catholicorum."

[10] Par. 60. "Studentes et eorum doctores vivent de bonis dictorum prioratum, etc."

The other element in his Utopia, which is to establish peace, is a modification of the processes of law (par. 90 f.). The processes must be shortened according to a definite plan; but the detail need not concern us here. The fact remains that he saw that social, educational and religious reform within the state are all means for the attainment of international peace.

The closing section of the work (110-142) are addressed to Philip, king of France, who is asked to send the preceding to Edward I. Dubois urges the economic gain from the abolition of wars, and in the meantime the institution of various military reforms—as for example the regular payment of troops. It is amusing to note that the author feels the danger to himself from the powers that be, if his projects are made too public. He therefore asks both Edward I and Philip to consider his ideas more or less privately; and he hints that one who does not happen to hold popular opinions may suffer even physical assault.

So far as we know nothing evil happened to Pierre Dubois. He was a lawyer who worked first for the king of France and afterward, when he wrote the *De recuperatione,* in the service of Edward I in Guyenne. He seems to have represented the central government in either case, and to have found his chief opponents among the churchmen. He is known as the author of a popular pamphlet in French against papal claims, as the writer of a few short Latin treatises, and as the elected representative of Coutances at the *Etats Généraux* which met in Tours in 1308. After that nothing is known of him.

More than six hundred years have gone since the treatise of Pierre Dubois was forgotten: and one may well rub one's eyes in wonder at what is now occurring in Europe. Perhaps we are dreaming. The practical man will say that the old plans for political reform are by current events proved to be valueless; that the internationalists are shown by the facts to be unable to understand real politics. And yet one would have thought that any plan might have been better worth trying than one which has brought us to our present pass. However that may be we should not despair too soon. Ecclesiastical reformation was suggested for hundreds of years before Europe arrived at the comparatively tolerant situation in religion now established. Political reformation may be more difficult, but the work of its forerunners is important. *Si Lyra non lyrasset, Lutherus non saltasset*: so also in politics, the effective reformer is taught by his predecessors who found the circumstances of their time too strong for them.

C. DELISLE BURNS.

LONDON, ENGLAND.

CLASS, FUNCTION, CONCEPT, RELATION.[1]

IN my *Grundlagen der Arithmetik* of 1884 I have tried to make it seem probable that arithmetic is a branch of logic and need not borrow any ground of proof whatever from experience or intuition. The actual demonstration of my thesis is carried out in my *Grundgesetze* of 1893 and 1903 by the deduction of the simplest laws of numbers by logical means alone. But to make this proof convincing, considerably higher claims must be made for deduction than is habitually done in arithmetic.[2] A set of a few methods of deduction has to be fixed beforehand, and no step may be taken which is not in accordance with them. Consequently, when passing over to a new judgment we must not be satisfied, as mathematicians seem nearly always to have been hitherto, with saying that the new judgment is evidently correct, but we must analyze each step of ours into the simple logical steps of which it is composed, —and often there are not a few of these new steps. No hypothesis can thus remain unnoticed. Every axiom which is needed must be discovered, and it is just the hypotheses which are made tacitly and without clear consciousness that hinder our insight into the epistemological nature of a law.

In order that such an undertaking be crowned with success, the concepts which we need must naturally be conceived distinctly. This is true especially in what concerns the thing that mathematicians denote by the word "aggregate" (*Menge*). It seems that Dedekind, in his book *Was sind und was sollen die Zahlen?*[3] of 1888, uses the word "system" to denote the same thing. But in spite of the exposition which appeared four years earlier in my *Grundlagen,* a clear insight into the essence of the matter is not to be found in Dedekind's work, though he often gets somewhat near it. This is the case in the sentence:[4] "Such a system S is completely determined if of everything it is determined whether it is an element of S or not. Hence the system S is the same as the system T (in symbols S = T) if every element of S is also element of T and every element of T is also element of S." In other passages, on the other hand, Dedekind strays from the point. For instance:[5] "It very frequently happens that for some reason different things *a, b, c,*... can be considered from a common point of view, can be put together in the mind, and we then say that they form a *system* S." Here a presentiment of the correct idea is contained in the words "common point of view"; but the "putting together in the mind" is not an objective characteristic. In whose mind, may I ask? If they are put together in one mind and not in another do they then form a system? What is to be put together in my mind must doubtless be in my mind. Then do not things outside myself form systems? Is a system a subjective formation in each single soul? Is then the constellation Orion not a system? And what are its elements? The stars, the molecules, or the atoms? The following sentence[6] is remarkable: "For uniformity of expression it is advantageous to admit the special case that a system S is composed of a *single* (one and one only) element *a*: the thing *a* is an element of S, but every thing different from *a* is not an element of S." This is afterward[7] understood in such a way that every element *s* of a system S can be itself regarded as a system. Since in this case element and system coincide, it is here quite clear that, according to Dedekind, the elements are the proper constituents of a system. Ernst Schröder in his lectures on the algebra of logic[8] goes a step in advance of Dedekind in drawing attention to the connection of his systems with concepts, which Dedekind seems to have overlooked. Indeed, what Dedekind really means when he calls a system a "part" of a system[9] is that a concept is subordinated to a concept or an object falls under a concept. Neither Dedekind nor Schröder distinguish between these cases because of a mistake in point of view which is common to them both. In fact, Schröder also, at bottom, considers the elements to be what really make up his *class*. An empty class should not occur with Schröder any more than an empty system with Dedekind. But the need arising from the nature of the matter makes itself felt in a different way with each writer. Dedekind says:[10] "On the other hand, we intend here for certain reasons wholly to exclude the empty system, which contains no element at all, although for other investigations it may be convenient to invent (*erdichten*) such a system." Thus such an invention is permitted; it is only desisted from for certain reasons. Schröder dares to invent an empty class. Apparently then both agree with many mathematicians in holding that we may invent anything we please that does not exist,—even what is unthinkable; for if the elements form a system, then the system is annulled at the same time as the elements. As to where the limits of this license lie and whether indeed there are any such limits, without any doubt we will not find much clearness and agreement;—and yet the correctness of a proof may depend on such questions. I believe I have settled them in a way that is final for all intelligent persons, in my *Grundlagen*[11] and in my lecture "Ueber formale Theorien der Arithmetik."[12] Schröder invents his zero-class and thus gets into difficulties.[13] We do not find, then, a clear insight into the matter with either Schröder or Dedekind; but still the true position of affairs is seen whenever a system is to be determined. Dedekind then brings forward properties which a thing must have in order to belong to a system, i. e., he defines a concept by its characteristics.[14] If now a concept is made up of characteristics and not of the objects falling under the concept, there are no difficulties to be urged against an empty concept. Of course in this case an object (*Gegenstand*) can never also be a concept, and a concept under which only one object falls must not be confused with this object. Thus we are finally left with the result that the number datum contains an assertion about a concept.[15] I have traced back number to the relation of similarity[16] (*Gleichzahligkeit*) and similarity to univocal correspondence (*eindeutige Zuordnung*). Of "correspondence" much the same holds as of "aggregate" (*Menge*). Nowadays both words are often used in mathematics, and

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[1] [Translated from the *Grundgesetze der Arithmetik* by Johann Stachelroth and Philip E. B. Jourdain.]

[2] *Grundlagen*, pp. 102-104.

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[3] [English translation under the title *Essays on the Theory of Numbers,* Chicago and London, 1901. See especially p. 45.]

[4] [*Ibid.*, p. 45.]

[5] [Ibid.]

[6] [*Ibid.*]

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[7] [*Ibid.*, p. 46.]

[8] *Vorlesungen über die Algebra der Logik* (*exakte Logik*), Vol. I, Leipsic, 1890, p. 253. [This reference of Frege seems wrong and it should perhaps rather be to such a page as p. 100. Cf. also Frege's later critical study: "Kritische Beleuchtung einiger Punkte in E. Schröders Vorlesungen über die Algebra der Logik," *Archiv für systematische Philosophie*, Vol. I, 1895, pp. 433-456.]

[9] [*Op. cit.*, p. 46:]

[10] [*Ibid.*, pp. 45-46.]

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[11] Pp. 104-108.

[12] *Sitzungsberichte der Jenaischen Gesellschaft für Medicin und Naturwissenschaft*, July 17, 1885.

[13] Cf. E. G. Husserl, *Göttinger gelehrte Anzeigen*, 1891, No. 7, p. 272,—where, however, the difficulties are not solved.

[14] On *concept, object, property,* and *characteristics*, cf. my *Grundlagen*, pp. 48-50, 60-61, 64-65, and my essay "Ueber Begriff und Gegenstand," *Vierteljahrsschrift für wissenschaftliche Philosophie*, Vol. XVI, 1892, pp. 192-205.

[15] See *Grundlagen*, pp. 59-60.

[16] [The same idea and word were used by Dedekind (*op. cit.*, p. 53); and the same idea but with the name "equivalence" was used by Georg Cantor (cf. *Contributions to the Founding of the Theory of Transfinite Numbers*, Chicago and London, 1915, pp. 40, 86).]

very often there is lacking an insight into what is intended to be denoted by them. If my opinion is correct that arithmetic is a branch of pure logic, then a purely logical expression has to be chosen for "correspondence." I choose the word "relation." *Concept* and *relation* are the foundation stones upon which I erect my structure.

But even when concepts have been grasped quite precisely, it would be difficult—nearly impossible in fact—to satisfy the demands we have had to make of a process of proof without some special means of help. Now such a means is my ideography (*Begriffsschrift*), the explanation of which will be my first problem. The following remarks may be noticed before we proceed farther. It is not possible to define everything, hence it must be our endeavor to go back to the logically simple which as such cannot properly be defined. I must then be satisfied with referring by hints to what I mean. Before all I have to strive to be understood, and therefore I will try to develop the subject gradually and will not attempt at first a full generality and a final expression. The frequent use made of quotation marks may cause surprise. I use them to distinguish the cases where I speak about the sign itself from those where I speak about its denotation. Pedantic as this may appear, I think it necessary. It is remarkable how an inexact mode of speaking or writing which perhaps was originally employed only for greater convenience or brevity and with full consciousness of its inaccuracy, may, when that consciousness has disappeared, end by confusing thought. Has it not happened that number signs have been mistaken for numbers, names for the things named, the mere auxiliary means for the real end of arithmetic? Such experiences teach us how necessary it is to make the highest demands of exactitude in manner of speech and writing. And I have taken pains at least to do justice to such demands wherever it seemed to be of importance.

If[17] we are asked to give the original meaning of the word "function" as used in mathematics, we easily fall into saying that a *function* of x is an expression formed by means of the notations for sum, product, power, difference, and so on, of "x" and definite numbers. This attempt at a definition is not successful because a function is here said to be an *expression,* a combination of signs, and not what the combination stands for. Then probably another attempt would be made with "denotation (*Bedeutung*) of an expression" instead of "expression." But there appears the letter "x" which indicates a number, not as the sign "2" does, but indefinitely. For different number-signs which we put in the place of "x", we get, in general, different denotations. Suppose for example, that in the expression "$(2+3.x^2)x$", instead of "x" we put the number-signs "0", "1", "2", "3", one after the other; we then get as corresponding denotations the numbers 0, 5, 28, 87. Not one of these denotations can claim to be our function. The essence of the function is in the correspondence that it establishes between the numbers whose signs we put for "x" and the numbers which then appear as denotations of our expression,—a correspondence which is represented to intuition by the course of the curve whose equation is, in rectangular coordinates, "$y=(2+3.x^2)x$". In general, then, the essence of the *function* lies in the part of the expression which is outside the "x". The expression of a function *needs completion* (*ist ergänzungsbedürftig*) and is *not satisfied* (*ungesättigt*). The letter "x" only serves to keep places open for a numerical sign which is to complete the expression, and thus makes known the special kind of need for completion that constitutes the peculiar nature of the function indicated above. In what follows, the Greek letter "ξ" will be used[18] instead of the letter "*x*". This keeping open is to be understood in this way: All places in which "ξ" stand must always be filled by the same sign and never by different ones. I call these places *argument-places* and that whose sign or name takes these places in a given case I call *argument of the function* for this case. The function is completed by the argument; I call what it becomes on completion the *value* of the function for the argument. We thus get a name of the value of a function for an argument when we fill the argument-places in the name of the function with the name of the argument. Thus, for example, "$(2+3.1^2)1$" is a name of the number 5, composed of the function-name "$(2+3.\xi^2)\xi$" and "1". The argument is not to be reckoned in with the function, but serves to complete the function which is unsatisfied by itself. If in the following an expression like "the $\Phi(\xi)$" is used, it is always to be observed that the only service rendered by "ξ" in the notation of the function is that it makes the argument-places recognizable; it does not imply that the essence of the function becomes changed when any other sign is substituted for "ξ".

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[17] Cf. my lecture *Funktion und Begriff,* Jena, 1891, and my essay "Ueber Begriff und Gegenstand" cited above. My *Begriffsschrift* of 1879 now no longer represents my standpoint, and thus should only be used with caution to illustrate what I said here.

To the fundamental operations of calculation mathematicians added, as function-forming, the process of proceeding to the limit as exemplified by infinite series, differential quotients and integrals; and finally the word "function" was understood in such a general way that the connection between value of function and argument was in certain circumstances no longer expressed by signs of mathematical analysis, but could only be denoted by words. Another extension consisted in admitting complex numbers as arguments and consequently also as function-values. In both directions I have gone still farther. While, indeed, the signs of analysis were hitherto on the one hand not always sufficient, they were on the other hand not all employed in the formation of function-names. For instance, "$\xi^2 = 4$" and "$\xi > 2$" were not allowed to count as names of functions; but I do so allow them. But that indicates at the same time that the domain of function-values cannot remain limited to numbers; for if I take as arguments of the function $\xi^2 = 4$ the numbers 0, 1, 2, 3, in succession, I do not get numbers. I get: "$0^2 = 4$", "$1^2 = 4$", "$2^2 = 4$", "$3^2 = 4$", which are expressions of one true and some false thoughts. I express this by saying that the value of the function $\xi^2 = 4$ is either the *"truth-value* (*Wahrheitswerth*) of the true or of the false."[19] From this it can be seen that I do not intend to assert anything by merely writing down an equation, but that I only designate (*bezeichne*) a truth-value, just as I do not intend to assert anything by simply writing down "2^2" but only *designate* a number. I say: "The *names* "$2^2 = 4$" *and* "$3 > 2$" *denote* the same truth-value" which I call for short *the true*. In the same manner "$3^2 = 4$" and "$1 > 2$" *denote* the same truth-value, which I call for short *the false* just as the name "2^2" *denotes* the number 4. Accordingly I say that the number 4 is the *"denotation"* of "4" and of "2^2", and that the true is the "denotation" of "$3 > 2$". But I distinguish the "meaning" (*Sinn*) of a name from its "denotation" (*Bedeutung*). The names "2^2" and "$2 + 2$" have not the same *meaning*, nor have "$2^2 = 4$" and "$2 + 2 = 4$". The meaning of the name of a truth-value I call a *"thought"* (*Gedanken*). I say further that a name *"expresses"* (*ausdrückt*) its meaning and *"denotes"* its denotation. I *"designate"* (*bezeichne*) by a name what it means.

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[18] Nothing, however, is fixed by this for our ideography. The "ξ" never appears in the developments of the ideography itself, and I only use it in my exposition of it and in illustrations.

The function $\xi^2 = 4$ can thus have only two values, the true for the arguments + 2 and — 2 and the false for all other arguments.

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[19] I have shown this more exhaustively in my essay "Ueber Sinn und Bedeutung" in the *Zeitschrift für Philos. und phil. Kritik,* Vol. C, 1892, pp. 25-50).

Also the domain of what is admitted as argument must be extended,—indeed, to objects quite generally. *Objects* (*Gegenstände*) stand opposed to functions. I therefore count as an object everything that is not a function; thus, examples of objects are numbers, truth-values, and the *ranges* (*Werthverläufe*) to be introduced further on. The names of objects—or *proper names*—are not therefore accompanied by argument-places, but are *satisfied* like the objects themselves.

I use the words, "the function $\Phi(\xi)$ has the same *range* as the function $\Psi(\xi)$", as denoting the same as the words, "the functions $\Phi(\xi)$ and $\Psi(\xi)$ have the same value for the same argument." This is the case with the functions $\xi^2 = 4$ and $3.\xi^2 = 12$, at least if numbers are taken as arguments. But we can also imagine the signs of evolution and multiplication defined in such a manner that the function $(\xi^2 = 4) = (3.\xi = 12)$ has the value of the true for any argument whatever. Here an expression of logic may be used: "The concept *square-root of* 4 has the same extension as the concept *something of which three times its square is* 12." With those functions whose value is always a truth-value we can therefore say "extension of the concept" instead of "range of the function," and it seems suitable to say that a *concept* (*Begriff*) is a function of which the value is always a truth-value.

Hitherto I have only dealt with functions of a single argument, but we can easily pass over to *functions with two arguments.* Such functions are doubly in need of completion. A function with one argument is obtained when a completion by means of one argument has been effected. Only by means of a repeated completion do we arrive at an *object,* and this *object* is then called the *"value"* of the function for the pair of arguments. Just as the letter "ξ" served with functions of one argument, I use here the letters "ξ" and "ζ" in order to indicate the twofold non-satisfaction of a function of two arguments, as, for example, in "$(\xi+\zeta)^2+\zeta$". By replacing "ζ" by "1", for example, we satisfy the function in such a way that we have in $(\xi+1)^2+1$ a function with only one argument. This manner in which we use the letters "ξ" and "ζ" must always be kept in mind when an expression like "the function $\Psi(\xi, \zeta)$" occurs.[20] I call the places in which "ξ" stands "ξ-argument-places", and those in which "ζ" stands "ζ-argument-places". I say that the ξ-argument-places are "*related*" (*verwandt*) to one another, and also the ζ-argument-places to one another, and I say that a ξ-argument-place is *not* related to a ζ-argument-place.

The functions with two arguments $\xi=\zeta$ and $\xi>\zeta$ have as value always a truth-value—at least if the signs "$=$" and "$>$" are defined in a suitable manner. I shall call such functions "relations". In the first relation, for example, 1 stands to 1, and in general every object to itself; in the second, for example, 2 stands to 1. I say that the object Γ "stands in the relation $\Psi(\xi, \zeta)$ to" the object Δ, if $\Phi(\Gamma, \Delta)$ is the true. I say that the object Δ "*falls under*" the concept $\Phi(\xi)$, if $\Phi(\Delta)$ is the true. It is presumed, of course, that both the functions $\Phi(\xi)$ and $\Psi(\xi,\zeta)$ have always truth-values as values.[21]

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I have already said above that no assertion is to lie as yet in a mere equation; by "$2+3=5$" only a truth-value is designated and it is not stated which of the two it is. Again, if I write "$(2+3=5)=(2=2)$" and presuppose that we know that $2=2$ is the true, yet I would not have asserted by that the sum of 2 and 3 is 5, but I would only have designated the truth-value of the circumstance that "$2+3=5$" denotes the same as "$2=2$". Thus we need a special sign to assert that something or other is true. For this purpose I write what I call a "sign of assertion" just before the name of the truth-value, so that if this sign is written just before "$2^2=4$",[22] it is asserted that the square of 2 is 4. I make a distinction between "*judgment*" (*Urtheil*) and "*thought*" (*Gedanken*), and understand by "judgment" the recognition of the truth of a "thought." I shall call the ideographic representation of a judgment by means of the sign of assertion an "ideographic theorem" or more shortly a "theorem." I regard this sign of assertion as composed of a vertical line, which I call "line of judgment" (*Urtheilsstrich*), and a short straight horizontal line proceeding from the middle of the vertical line and going toward the right, which I will simply call the "horizontal line" (*Wagerechte*). In my *Begriffsschrift* I called this last line the "line of content" (*Inhaltsstrich*) and at that time I expressed by the words "judicable content" (*beurtheilbarer Inhalt*) what I have now arrived at distinguishing into truth-value and thought.[23] The horizontal line most often occurs in combination with other signs, as it does here with the line of judgment, and is thus guarded against confusion with the minus sign. Wherever it occurs by itself it must be made somewhat longer than the minus sign for purposes of distinction. I regard it as a name of a function in the way that "Δ" preceded by this sign denotes the true if Δ is the true, and the false if Δ is not the true. Of course the sign "Δ" must denote an *object*; names without denotation may not occur in our ideography. The above arrangement is made so that "Δ" preceded by a horizontal line denotes something under all circumstances if only "Δ" denotes something. If not, "ξ" preceded by a horizontal line would not denote a concept with sharp boundaries,—and thus would not denote a *concept* in my sense. I here use capital Greek letters as names denoting something without my saying what their denotations are. In the actual developments of my ideography they will not occur any more than "ξ" and "ζ". The above "ξ" preceded by a horizontal line denotes a function whose value is always a truth-value or, by what I have said, a concept. Under this concept falls the true and this only. Thus "$2^2=4$" preceded by a horizontal line denotes the same thing as "$2^2=4$", namely the true. In order to do away with brackets, I lay down that all which stands to the right of the horizontal line is to be regarded as a whole which stands at the argument-place of the function denoted by "ξ" preceded by a horizontal line, unless *brackets* forbid this. The sign "$2^2=5$" preceded by a horizontal line denotes the false and thus the same as "$2^2=5$", whereas "2" preceded by a horizontal line denotes the false, and thus something different from the number 2. If "Δ" is a truth-value, Δ preceded by a horizontal line is the same truth-value, and thus the equation of "Δ" to "Δ" preceded by a horizontal line denotes the true. But this equation denotes the false is Δ is not a truth-value; so that we can say that it denotes the truth-value of the circumstances that Δ is a truth-value.

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[20] Cf. note 18.

[21] Here there is a difficulty which may easily obscure the true position of things and thus rouse distrust of the correctness of my view. If we compare the expression "the truth-value of the circumstance that Δ falls under the concept $\Phi(\xi)$" with "$\Phi(\Delta)$", we see that to the "$\Phi(\Delta)$" properly corresponds "the truth-value of the circumstance that (Δ) falls under the concept $\Phi(\xi)$" and not "the concept $\Phi(\xi)$". The last words do not therefore really designate a concept (in my sense of the word), though they have the appearance of doing so in our linguistic form. With regard to the constrained position in which language here finds itself, cf. my essay "Ueber Begriff und Gegenstand" mentioned in note 14.

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[22] I often use here the notations of sum, product, and power in order conveniently to form examples and to facilitate understanding by means of hints, although these signs are not yet defined in this place. But we must keep in view the fact that nothing is founded on the denotations of these signs.

[23] Cf. my essay "Ueber Sinn und Bedeutung" cited above.

Thus the function "$\Phi(\xi)$" preceded by a horizontal line, denotes a concept and the function "$\Psi(\xi,\zeta)$" preceded by a horizontal line, denotes a relation, whether or not $\Phi(\xi)$ is a concept and $\Psi(\xi, \zeta)$ is a relation.

Of the two signs out of which the sign of assertion is composed the line of judgment alone contains the assertion.

We need no sign to declare that a truth-value is the false, if only we have a sign by which either truth-value is changed into the other. This sign is also indispensable on other grounds. I now lay down that the value of the function denoted by "ξ" preceded by a horizontal line from the middle of which hangs a small vertical line directed downward and called the "line of denial" (*Verneinungsstrich*), so that the whole is like a sign of assertion turned round on its face, is to denote the false for every argument for which the value of the function denoted by "ξ" preceded by a horizontal line is the true. For all other arguments the function under definition is to be the true. The function thus defined may be called "the negation of ξ", and thus its value is always a truth-value; it is a concept under which all objects fall with the single exception of the true. From this it follows that horizontal lines, whether or not they form part of a sign of negation, can be combined with immediately preceding or following simple horizontal lines in such a way that the latter, so to speak, lose their separate existence and *melt into* the former (*Verschmelzung der Wagerechten*).

Thus "the negation of $2^2 = 5$" denotes the true; and thus we may put the sign of assertion so as to join on to the left of the sign of negation. We may assert, too, the negation of 2.

I have already used the sign of equality to form examples, but it is necessary to lay down something more accurate about it. The sign "$\Gamma = \Delta$" is to denote the true if Γ is the same as Δ, and the false in all other cases.

In order to dispense with brackets as far as possible, I lay down that all which stands on the left of the sign of equality as far as the nearest horizontal line is to denote the ξ-argument of the function $\xi = \zeta$, in so far as *brackets* do not forbid this; and that all which stands on the right of the sign of equality as far as the next sign of equality is to denote the ζ-argument of that function in so far as *brackets* do not forbid this.

GOTTLOB FREGE.

JENA, GERMANY.

A CHINESE POET'S CONTEMPLATION OF LIFE.

INTRODUCTION.

MY attention has repeatedly been called to the poetry of Su Tung P'o (also briefly named "Su Hsi"), especially to his thoughtful meditation on an excursion by boat to the Scarlet Cliff. In this poem he comments on the transiency of life, and referring to the law of change as represented by the phases of the moon he finds the underlying permanence symbolized by the river which remains the same although its waters pass on without a halt.

The original was kindly furnished me by Mr. Sawland J. Shu, president of the Technological College at Nanking, while a literal translation was procured through Prof. Frederick G. Henke from Mr. W. T. Tao and another one from Prof. King Shu Liu, of the University of Nanking. Professor Henke further informed me on the authority of Prof. William F. Hummel that a prose translation by Prof. Herbert A. Giles was published in the *University of Nanking Magazine* and republished together with other Chinese poems collected in the volume entitled *Gems of Chinese Literature.*

Professor Giles says that Su Tung P'o was "even a greater favorite with the Chinese literary public" than the famous Ou-Yang Hsiu.[1] So we may regard Su Tung P'o as easily a genius of first rank. Professor Giles says of him:

"Under his hands, the language of which China is so proud may be said to have reached perfection of finish, of art concealed. In subtlety of reasonings, in the lucid expression of abstractions, such as in English too often elude the faculty of the tongue, Su Tung P'o is an unrivalled master."

Even a rough translation of his poems will impress the reader with the versatility as well as the profundity of his poetic flights, and here I venture to present his famous poem on "The Scarlet Cliff" in English blank verse which seems to be the appropriate form for this kind of thought. I hope that it will be a fair example of Chinese literature in its noblest accomplishment.

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[1] Ou-Yang Hsiu lived 1017-1072 A. D. Professor Giles says of him: "A leading statesman, historian, poet, and essayist of the Sung dynasty. His tablet is to be found in the Confucian temple, an honor reserved for those alone who have contributed to the elucidation or dissemination of Confucian truth."

There are some people who have little appreciation of the beauties of Chinese literature and have nothing but ridicule or even contempt for it. With reference to one of these haughty scoffers Professor Giles adds with grim humor:

"On behalf of his (Su Tung P'o's) honored manes I desire to note my protest against the words of Mr. Baber, recently spoken at a meeting of the Royal Geographical Society, and stating that 'the Chinese language is incompetent to express the subtleties of theological reasoning, just as it is inadequate to represent the nomenclature of European science.' I am not aware that the nomenclature of European science can be adequately represented even in the English language; at any rate, there can be no comparison between the expression of terms and of ideas, and I take it the doctrine of the Trinity itself is not more difficult of comprehension than the theory of 'self-abstraction beyond the limits of an external world,' so closely reasoned out by Chuang Szu. If Mr. Baber merely means that the gentlemen entrusted with the task have proved themselves so far quite incompetent to express in Chinese the subtleties of theological reasoning, then I am with him to the death."

Mr. K. S. Liu sends with his translation these further remarks concerning Su Hsi, the classical philosopher of Chinese *belles lettres*:

"This poem was composed by Su Hsi, a famous Chinese poet who flourished 1036-1101. Owing to the intrigues of his political enemies he was exiled to Hwang-Cheo, a place in the province of Hu Peh. While there he made a visit to a place called Chi Pi (literally Red Wall), made famous by the battle which took place there between Tsao-tsao and Cheo-yu (two historical characters in the period of the Three Kingdoms). The poem is an account of this visit and a description of the feelings it aroused in him. Like many other poets who consider poetry an embodiment in symbols of one's inner spiritual experiences, he shows in the poem, first, the ephemeral nature of human existence with all its paraphernalia, and then how in the contemplation of nature one can transcend the mutations of time and be one with the eternal order. In this state one can rise above the vicissitudes of life."

The poem begins by giving the date of Su Hsi's excursion to the Scarlet Cliff. The year reads in Chinese characters *jan süh,* and we here encounter the difficulty of reproducing the Chinese method of determining chronology. For this they make use of the sexagenary cycle by repeating five times the twelve branches and six times the ten stems (see the author's *Chinese Thought,* p. 4).

1 前游赤壁賦　蘇軾
2 壬戌之秋七月既望蘇子與客泛舟
3 游於赤壁之下清風徐來水波不興
4 舉酒屬客誦明月之詩歌窈窕之章
5 少焉月出於東山之上徘徊於斗牛
6 之間白露橫江水光接天縱一葦之
7 所如凌萬頃之茫然浩浩乎如馮虛
8 御風而不知其所止飄飄乎如遺世
9 獨立羽化而登僊於是飲酒樂甚扣
10 舷而歌之歌曰桂棹兮蘭槳擊空明
11 兮泝流光渺渺兮予懷望美人兮天
12 一方客有吹洞簫者倚歌而和之其
13 聲嗚嗚然如怨如慕如泣如訴餘音
14 嫋嫋不絕如縷舞幽壑之潛蛟泣孤
15 舟之嫠婦蘇子愀然正襟危坐而問
16 客曰何為其然也客曰月明星稀烏

CHINESE TEXT.

The meaning of *jan* (pronounced *zhan*) is the "germ in the womb," and it "denotes the ninth of the ten stems; it is connected with the north and running water." It means "great, full" and also "to flatter and adulate." As the ninth of the ten stems it denotes swollen water, hence we translate it "billow." The other character *süh* which is the eleventh of the twelve branches denotes in its

17 鵲南飛此非曹孟德之詩乎東望夏
18 口西望武昌山川相繆鬱乎蒼蒼此
19 非孟德之困於周郎者乎方其破荊州
20 下江陵順流而東也舳艫千里旌旗蔽
21 空把酒臨江橫槊賦詩固一世之雄也而
22 今安在哉況吾與子漁樵於江渚之上侶
23 魚蝦而友麋鹿駕一葉之扁舟舉匏尊
24 以相屬寄蜉蝣於天地渺滄海之一粟

25 哀吾生之須臾羨長江之無窮挾飛
26 僊以遨遊抱明月而長終知不可乎
27 驟得託遺響於悲風蘇子曰客亦知夫
28 水與月乎逝者如斯而未嘗往也盈虛
29 者如彼而卒莫消長也蓋將自其變者
30 而觀之則天地曾不能以一瞬自其不變
31 者而觀之則物與我皆無盡也而又何羨
32 乎且夫天地之間物各有主苟非吾之所

CHINESE TEXT.

horary significance the hour 7-9 P. M., called the "dog hour." We here translate it by "hound." To Chinamen this denotation of the year is very familiar, but it is difficult to reproduce its exact significance in a poetic translation in English. The "billow hound" year corresponds in our chronology to 1082 A. D., which is the fifty-eighth year in the sexagenary cycle under the Sung dynasty. The latter being a matter of course in the poet's day is not mentioned in the Chinese text.

33 有雖一毫而莫取唯江上之清風山間
34 之明月耳得之而為聲目遇之而成色
35 取之不盡用之不竭是造物者之無盡
36 藏也而吾與子之所共適客喜而笑洗
37 盞更酌肴核既盡杯盤狼藉相與枕
38 藉乎舟中不知東方之既白
39 千九百十四年六月宋旦仕鴻島書

CHINESE TEXT.

The songs "To the Bright Moon" and "To the Modest Maid" mentioned in the poem are probably the odes known as I, XII, 8 and I, III, 17 of the *Shih King,* the canonical collection of ancient Chinese songs. In the translation of William Jennings (*The Shi King,* pages 151 and 69) they read as follows:

To the Bright Moon.

O Moon that climb'st effulgent!
O ladylove most sweet!

Would that my ardor found thee more indulgent!
  Poor heart, how dost thou vainly beat!

O Moon that climb'st in splendor!
  O ladylove most fair!
Couldst thou relief to my fond yearning render!
  Poor heart, what chafing must thou bear!

O Moon that climb'st serenely!
  O ladylove most bright!
Couldst thou relax the chain I feel so keenly!
  Poor heart, how sorry is thy plight!

To the Modest Maid.

A modest maiden, passing fair to see,
Waits at the corner of the wall for me.
I love her, yet I have no interview:—
I scratch my head—I know not what to do.

The modest maid—how winsome was she then,
The day she gave me her vermilion pen!
Vermilion pen was never yet so bright—
The maid's own loveliness is my delight.

Now from the pasture lands she sends a shoot
Of couchgrass fair; and rare it is, to boot.
Yet thou, my plant (when beauties I compare),
Art but the fair one's gift, and not the Fair!

There is some doubt, according to Professor Giles, whether the Scarlet Cliff visited by Su Hsi was really the place of battle as the latter assumes, but the poem remains of the same significance even if Su Hsi was mistaken, and we need feel no concern about it.

P. C.

THE SCARLET CLIFF.

It was the Billow-Hound year of House Sung:
The seventh moon was on the wane, when I
Was down stream drifting in a boat with friends
On an excursion to the Scarlet Cliff.

The evening breeze so gently blew that scarce
The water rippled on its smooth expanse.
I filled the cups and bade my friends to sing
The ode "To the Bright Moon," and then they chanted
The lay melodious "To the Modest Maid."

Slowly the moon rose o'er the eastern hills,
And passed between the Wain and Capricorn,
Shedding her silver beams upon the water,
To link our world below with heaven above.

In such surroundings, infinite in charm,
Our skiff was freely gliding,—traveling
Unchecked through space, unmindful whither bound;
Like gods we moved in a transcendent realm:
I poured out a libation for our joy,
And beating time on our boat's wooden rim,
I sang these verses in sad exaltation:

"Our olive boat with orchid oars propelled,
  Breaks splashing through the moonlit glittering
    wave;
  In lovelorn loneliness I here am held,
  From friends who now lie buried in the grave."

One of my guests accompanied the song
Upon his flageolet, with proper notes
To suit the music to the sentiment
Of plaintive moods, in sounds that wove unbroken
Their silken threads around our company.
The music stirred the dragon in the deep
And moved the the boatswain's widow unto tears.
"And why is that?" I asked in pensive query
My cherished guest. "Why does thy magic art
So powerfully affect us all?" Said he:

"Few stars are seen and yet the moon shines bright,
To southern lands the raven wings his flight.

"Was this not uttered here by Tsao Meng Te,
Here, eastward of Hsia-K'ou, west of Wu-Chang,
Where hill and stream in wild luxuriance blend?
'T is here Meng Te was routed by Chou Yü.
Before him lay Ching-chou. Kiang-ling he conquered,
And eastward did he push upon the river;
His warships, prow to stern, stretched thousand miles,
The banners of his troops darkened the sun.
Then a libation he poured out, and nearing
The Scarlet Cliff, the hero of his age,
On horseback, clad in armor, spake those words!
Yet where is he to-day? And what are we?
To-day we fish and gather fuel here
On river isles where shrimps are our companions
And deer our friends. We paddle here about
In frail canoe and drink companionship
From flasks of gourd. How transient is the life
Of creatures as ephemeral as we.
Tossed o'er the ocean like a husk of straw,
We are mere twinklings on the river Time;
Oh, could I be the stream itself which rolls
Incessantly and without end! Alas!
Could I but clasp the bright and beauteous moon
Close to my heart and dwell with her in heaven!
Yet unfulfilled remain my deep-felt yearnings
Which find expression in melodious strain."

"But you my friend," replied I questioning,
"Do you well comprehend the mystery
Of this great river and the changing moon?
Past flows the water but 'tis never gone;
The moon is waning, but again 'twill wax.

So I with this great world, all in a change—
E'en Heaven and Earth are transient constantly—
Myself, and also thou, in this same sense
Viewed as a whole, live on eternally.
Why then lament? Thou long'st for what thou hast!"
And further musing on life's complex problems
Continued I: "Whate'er our senses hold
Is owned by him who feels it, who enjoys it.
For nothing can I take unless I own it,
The bracing breeze, the landscape of the river,
The moon above the valleys, gorgeous sights
Enrapturing the eye, and all the sounds
Which greet the ear, all are enjoyed by me.
All these are mine, and without let or hindrance
Are they the gifts of God, unstintedly
Given to man—indeed to all mankind.
And we enjoy them now."
He smiled approval—
My friend; he threw away the dregs of wine
And had his cup refilled up to the brim.

Thus finishing our feast we laid us down
To rest among the scattered cups and plates,
While in the distant east dim streaks of light
Appeared as heralds of another day.

CRITICISMS AND DISCUSSIONS.

LEIBNIZ AND LOCKE.

John Locke, the founder of the sensationalist school, who formulated the principle of his philosophy in the statement *Nihil est in intellectu quod non antea fuerit in sensu,* and who therefore on the one hand denied innate ideas and on the other claimed that all knowledge rises from experience, devotes to an investigation of truth Chapter V, and also part of Chapter VI of his famous work *On the Human Understanding* from which we make the following extracts:

" 'What is truth?' was an inquiry many ages since; and it being that which all mankind either do or pretend to search after, it cannot but be worth our while carefully to examine wherein it consists; and so acquaint ourselves with the nature of it, as to observe how the mind distinguishes it from falsehood.

"Truth then seems to me, in the proper import of the word, to signify nothing but the joining or separating of signs, as the things signified by them do agree or disagree one with another. The joining or separating of signs here meant, is what by another name we call 'proposition.' So that truth properly belongs only to propositions: whereof there are two sorts, viz., mental and verbal; as there are two sorts of signs commonly made use of, viz., ideas and words...

"We must, I say, observe two sorts of propositions that we are capable of making:

"First, Mental, wherein the ideas in our understandings are, without the use of words, put together or separated by the mind perceiving or judging of their agreement or disagreement.

"Secondly, Verbal propositions, which are words, the signs of our ideas, put together or separated in affirmative or negative sentences. By which way of affirming or denying, these signs, made by sounds, are, as it were, put together or separated one from another. So that proposition consists in joining or separating these signs, according as the things which they stand for agree or disagree....

"When ideas are so put together or separated in the mind, as they or the things they stand for do agree or not, that is, as I may call it 'mental truth.' But truth of words is something more, and that is the affirming or denying of words of another, as the ideas they stand for agree or disagree: and this again is twofold; either purely verbal and trifling or real and instructive, which is the object of real knowledge....

"Though our words signify nothing but our ideas, yet being designed by them to signify things, the truth they contain, when put into propositions, will be only verbal when they stand for ideas in the mind have not an agreement with the reality of things. And therefore truth, as well as knowledge, may well come under the distinction 'verbal' and 'real'; that being only verbal truth wherein terms are joined according to the agreement or disagreement of the ideas they stand for, without regarding whether our ideas are such as really have or are capable of having an existence in nature. But then it is they contain real truth when these signs are joined as our ideas agree; and when our ideas are such as we know are capable of having an existence in nature: which in substances we cannot know but by knowing that such have existed.

"Truth is the marking down in words the agreement or disagreement of ideas as it is. Falsehood is the marking down in words the agreement or disagreement of ideas otherwise than it is. And so far as these ideas thus marked by sounds agree to their archetypes, so far only is the truth real. The knowledge of this truth consists in knowing what ideas the words stand for, and the perception of the agreement or disagreement of those ideas, according as it is marked by those words....

"Certainty is twofold; certainty of truth, and certainty of knowledge. Certainty of truth is, when words are so put together in propositions as exactly to express the agreement or disagreement of the ideas they stand for, as really it is. Certainty of knowledge is, to perceive the agreement or disagreement of ideas, as expressed in any proposition. This we usually call 'knowing,' or 'being certain of the truth of any proposition.'"

His great critic Leibniz wrote a voluminous book[1] to refute

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[1] *New Essays Concerning Human Understanding.* Translated by A. G. Langley. 2d ed., Chicago and London, 1916.

Locke's sensationalism, pointing out that what Locke called reflection was not a product of sensation. He amended Locke's principle to read: *Nihil est in intellectu quod non antea fuerit in sensu, nisi intellectus ipse*, and this amendment upset Locke's very lucid but superficial arguments. According to Leibniz the senses furnish us the material for positive knowledge but they offer nothing but particular instances, not methods, nor principles, nor general truths. Brutes have the same sensations as man, but brutes can never attain to necessary propositions. These conceptions of necessary propositions are innate in the human mind. The human mind is not a *tabula rasa,* but contains certain principles which, in the measure that experience furnishes the occasion, develop into ideas of eternal and necessary verities.

From this standpoint Leibniz distinguishes two kinds of truths, necessary truths and contingent truths; the former are the eternal verities as instanced by mathematics, the latter the knowledge of particular facts furnished by experience. God is the ultimate source of both kinds of truth; the eternal verities correspond to his intellect, the contingent truths to his will. The former are such and can not be different because God is such; the latter could be different but are not because God willed them to be as they are and not otherwise. Necessary truths reveal to us what is possible and what impossible. Thus, e. g., a regular decahedron (i. e., a figure bounded by ten equal plane surfaces) is impossible, and "all intelligible ideas have their archetype in the eternal possibilities of things."

In reply to Locke's view of certainty, Leibniz says:

"Our certitude would be small, or rather nothing, if it had no other basis of simple ideas than that which comes from the senses. Have you forgotten, sir, how I have shown that ideas are originally in our mind, and that indeed our thoughts come to us from the depths of our own nature, other creatures being unable to have an immediate influence upon the soul? Besides, the ground of our certitude in regard to universal and eternal truths is in the ideas themselves, independently of the senses, just as ideas pure and intelligible do not depend on the senses, for example, those of being, unity, identity, etc. But the ideas of sensible qualities, as color, savor, etc., (which in reality are only phantasms) come to us from the senses, i. e., from our confused perceptions. And the basis of the truth of contingent and particular things is in the succession which causes these phenomena of the senses to be rightly united as the intelligible truths demand."

It is not our intention to criticize any one of the philosophers but we wish to point out how far and in what respect we agree with Leibniz's views as here outlined. We select Leibniz because his philosophy is less onesided than any other and has incorporated all considerations, religious, scientific, mathematical and historical. What he calls innate ideas reflecting the eternal and necessary truths whose source lies in God, we denote as the purely formal and we have shown that purely formal conceptions have been gained by abstraction. Man alone has the faculty of abstraction and so he alone is capable of producing and operating with purely formal conceptions such as numbers, geometrical figures, the notion of mathematical or pure space, logical syllogisms, the formulas of causation and of the conservation of substance and energy. The principle pervading the function of these concepts is called reason, and reason truly reflects the cosmic order, which is due to the efficiency of purely formal interrelations—the so-called purely formal laws. Our senses furnish us particulars only, and these particulars, which are innumerable isolated sense-impressions, would remain a chaos of disconnected items if they were not classified and systematized according to purely formal laws. The point overlooked by Leibniz and also later on by Kant is the question as to the origin of mind. The framework of reason, man's logical faculty, his notion of numbers and of space relations have indeed originated through experience as Locke claimed, but it was experience in a wider sense than either Locke or Leibniz conceived it to be. Experience in those days meant sense-experience, or the purely sensory element of sentient creatures. In this sense Leibniz is right that no amount of sense-impressions can bring forth an eternal or universal or necessary idea. Locke on the other hand, conscious of the fact that man was in possession of universal and necessary concepts and admitting no other source of knowledge than experience, insisted on the proposition that all ideas, even the most complicated ones, were derived from sensations, as which he understands experience to be.

Now it is obvious that there is nothing purely sensory, Sensations are possessed of forms and the formal impresses itself together with sense impressions upon sentient creatures. We have on other occasions set forth how sensory impressions are by a mechanical necessity so grouped that they are registered together, the particular ones being subsumed under the more general so that all of them build up a well-arranged system constituting a logical framework of types. This framework is the mind which is built up not of mere sensations, but of the interrelations of sense-impressions according to their various forms. Experience in the current sense includes the form of the sensory, and in this sense the faculty of conceiving purely formal relations has indeed arisen from experience.

The sensationalist school identifies the sense element of our knowledge with the formal and overlooks their radical difference. We must insist against the sensationalist school that everything formal is radically different from the sensory. The sensory is always particular while the formal can be generalized. By leaving out of sight everything particular our thought can operate in a field of pure relations, and we can exhaust all their possibilities. We can say what is possible as well as what is impossible and (all interference of unexpected particulars being excluded) we can also say what result will always be obtained under definite given conditions. We can exhaust all possibilities of the purely formal and can systematize the whole field. What will always be, is called "necessary," and so these propositions which are inevitable are called by Leibniz "eternal truths."

We agree with Leibniz that the source of these eternal truths is God; nay we go one step further in definiteness and claim that the eternal verities, of which our human notions of eternal truths are mental reflections, are God himself. All depends on our definition of God. Together with the whole cosmic order the necessary truths constitute an eternal omnipresence, an efficient system of norms which mould the world and determine all things. They form a kind of spiritual, or purely formal organism, a superpersonal presence which is the ultimate *raison d'être* and determinant of all things, the cosmos in its entirety as well as all particular events that happen in the course of its being.

Any one who has once grasped the deep significance of the purely formal will have liberated his mind forever of the superstitious, mystical or allegorical conceptions of the deity, but he will at the same time understand the truth that underlies the God-idea and thus he will know the real nature of the true God, whose existence is not a matter of belief, but a scientific certainty. All former proofs of the existence of God were necessarily failures, because in all cases the attempt was made to prove the existence of an anthropomorphic God with arguments that prove the true God, the eternal norm of being, and here the argument breaks down, because it no longer applies to the idea of an anthropomorphic God.

Leibniz has not overcome the mystical conception both of God and truth. He has unfortunately adopted the very primitive conception of an atomic nature of reality which is described in his monadology. It is strange that a man of his caliber did not see how contradictory is the idea of God as the central monad. On the other hand his theory is vindicated if we interpret his God to be the universal and omnipresent norm that regulates every event and constitutes the cosmic order of the world.

Insisting on the unity of the soul, Leibniz conceived all unities as local units, and these innumerable local units, the monads, were conceived as centers of force endowed with feeling and an entelechy, which means that they were capable of pursuing purposes. At the same time Leibniz held them to be separate entities, so as to render their cohesion and interaction a profound problem which could be solved only by the bold hypothesis of the preestablished harmony.

The problem of unity together with all problems of combination and configuration belongs in the domain of pure form. Combination of several parts working in cooperation constitute a unity and introduce something new. It did not exist before and will break to pieces again, but the law of its combination remains forever and constitutes the eternal background of its existence. The sensationalist school misses the main point of all philosophical considerations and thus loses the essence of the significance of religion; but Leibniz who discovers the weak spot in their arguments has not succeeded in persenting a satisfactory solution of the problem but ends in proclaiming a mystical God-conception and a dogmatic proclamation of a preestablished harmony. P. C.

EXISTENTS AND ENTITIES.[1]

That we must distinguish between what we may call "having existence" and "having entity or being" becomes evident when we look somewhat closely at ordinary mathematical propositions. A class (or system, or aggregate) M is said to "exist" when it has at least one member;[2] whereas, when mathematicians speak of, for example, "the existence of roots of an equation" or "the existence of the definite integral of a continuous function," they use the word "existence" in another sense: the roots or the integral are not classes, but individuals constructed out of mathematical concepts to supply an answer to certain questions. We can, of course, consider such an individual as *the* member of the class (N) whose sole member is this individual, and can then consider the second kind of mathematicians' existence-proofs as proofs of the existence of the class N; but we should, for the sake of clearness, avoid speaking of the "existence" of the member[3] of N, and use some such word as "entity" or "being" instead.

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[1] Cf. *Monist*, Jan. 1910, Vol. XX, p. 114, note 85.

Mr. B. Russell[4] has thus distinguished *being* and *existence* in 1901: "*Being* is that which belongs to every conceivable term, to every possible object of thought—in short to everything that can possibly occur in any proposition, true or false, and to all such propositions themselves. Being belongs to whatever can be counted. If A be any term that can be counted as one, it is plain that A is something, and therefore that A is. 'A is not' must always be either false or meaningless. For if A were nothing, it could not be said not to be; 'A is not' implies that there is a term A whose being is denied, and hence that A is. Thus unless 'A is not' be an empty sound, it must be false—whatever A may be, it certainly is. Numbers, the Homeric gods, relations, chimeras, and four-dimensional spaces all have being, for if they were not entities of a kind, we could make no propositions about them. Thus being is a general attribute of everything, and to mention anything is to show that it is.

"*Existence*, on the contrary, is the prerogative of some only amongst beings. To exist is to have a specific relation to existence—a relation, by the way, which existence itself does not have. This shows, incidentally, the weakness of the existential theory of judgment—the theory, that is, that every proposition is concerned with something that exists. For if this theory were true, it would still be true that existence itself is an entity, and it must be admitted that existence does not exist. Thus the consideration of existence itself leads to non-existential propositions, and so contradicts the theory...."

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[2] Cf., e. g., Dedekind, *Was sind und was sollen die Zahlen?* 2d ed., Braunschweig, 1893, pp. 5, 12; or *Essays on the Theory of Numbers*, Chicago, 1901, pp. 49, 58; Russell, *The Principles of Mathematics*, Cambridge, 1903, pp. 21, 32.

[3] Of course, the member of N may be itself a class and may thus "exist," but we obviously need not consider this further.

[4] *Mind*, N. S., Vol. X, No. 39, 1901, pp. 310-311.

This doctrine was repeated in Mr. Russell's *Principles of Mathematics*;[5] the existence-theorems of mathematics were said[6] to be "proofs that the various classes defined are not null," and the earlier statement[7] that these theorems are proofs "that there are entities of the kind in question" must not be taken to mean what it apparently expresses.

While Mr. Russell emphasized the distinction between *entity* and *existence*, it does not seem that at that time he quite realized the full bearings of the question, at least in mathematics. He attributed a denotation to every term that can possibly occur in a proposition. Thus "the round square" had a denotation, and the only further existence-question in logic and mathematics was whether the numbers—at least such as were defined as classes—, classes of spaces, and so on, could be proved to "exist,"—whether members of the classes in question could be constructed by logical methods provided that the initial postulates are granted.

* * *

Before going on to discuss the clear separation of the important question of entity from the less important question of existence, which came in Mr. Russell's later works, we will refer to the very strong tendency, even among logicians and mathematicians, to attribute a denotation to every denoting phrase.

Thus, H. MacColl[8] remarked that a symbol which corresponds to nothing in our universe of admitted realities, has, nevertheless, "like everything else named," a *symbolical* entity. In his sixth paper on "Symbolic Reasoning,"[9] MacColl attempted to give a simple theory of the existential import of propositions.

By $e_1, e_2, e_3, \ldots$, he denoted "our universe of *real existences*," and by $o_1, o_2, o_3, \ldots$, "our universe of *non-existences*, that is to say, of *unrealities*, such as *centaurs, nectar, ambrosia, fairies,* with self-contradictions, such as *round squares, square circles, flat spheres,* etc."; the "symbolic universe, or universe of discourse," S, may consist either wholly of realities, wholly of unrealities, or partly of realities and partly of unrealities. . . . If A denotes an individual or a class, any intelligible statement $\phi(\mathrm{A})$ containing the symbol A, implies that the individual or class represented by A has a *symbolic* existence; but whether the statement $\phi(\mathrm{A})$ implies that that which A denotes has a *real* or *unreal* or (if a class) partly real and partly unreal existence, depends upon the context."

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[5] Pp. 449-450; cf. pp. 43, 71.

[6] *Ibid.*, p. 497.

[7] *Ibid.*, p. vii.

[8] *Symbolic Logic and its Applications*, London, 1906, p. 42; MacColl here and elsewhere used the word "existence" where we use "entity." Cf. *Mind*, N. S., Vol. XI, 1902, pp. 356-357.

[9] *Mind*, N. S., Vol. XIV, 1905, pp. 74-81; cf. *Symbolic Logic and its Applications*, pp. 5, 76-78.

We will pass over the discussion between Messrs. MacColl and A. T. Shearman[10] on the interpretation of the Boolian equation "O=OA," and come to Mr. Russell's articles of 1905,[11] in which the theory of non-entity was, it seems, for the first time treated satisfactorily.

The sense in which the word "existence" is used in symbolic logic is a definable and purely technical sense. To say that A exists means that A is a class which has at least one member. Thus whatever is not a class does not exist in this sense; and among classes there is just one that does not exist, namely, the null-class. MacColl's two universes of existences and non-existences are not to be distinguished in symbolic logic, and each of them is identical with the null-class. There are no centaurs; "x is a centaur" is false whatever value we give to x, even when we include values which do not "exist" in the meaning which occurs in philosophy and daily life, such as numbers or propositions.

"The case of nectar and ambrosia is more difficult, since these seem to be individuals, not classes. But here we must presuppose definitions of nectar and ambrosia: they are substances having such and such properties, which, as a matter of fact, no substances do have. We have thus merely a defining concept for each, without any entity to which the concept applies. In this case, the concept is an entity, but it does not denote anything. . . . These words [such as nectar and ambrosia] have a *meaning*, which can be found by looking them up in a classical dictionary, but they have not a *denotation*: there is no entity, real or imaginary, which they point out."

[10] *Mind*, N. S., Vol. XIV, 1905, pp. 78-79, 295-296, 440, 578-580; Vol. XV, 1906, pp. 143-144; and Shearman's book *The Development of Symbolic Logic; a Critical-Historical Study of the Logical Calculus*, London, 1906, pp. 161-171.

[11] "The Existential Import of Propositions," *Mind*, N. S., Vol. XIV, 1905, pp. 398-401; "On Denoting," *ibid.*, pp. 479-493.

The last sentence refers to Frege's[12] distinction of *Sinn* (meaning) and *Bedeutung* (denotation).

A point of passing interest in connection with an attempt at the solution of a mathematical paradox, referred to later, is this sentence in MacColl's reply:[13] "I may mention, as a fact not wholly irrelevant, that it was in the actual application of my symbolic system to concrete problems that I found it absolutely necessary to label realities and unrealities by special symbols e and o, and to break up the latter class into separate individuals, o_1, o_2, o_3, etc., just as I break up the former into separate individuals e_1, e_2, e_3, etc."

When a phrase which in form is denoting, and yet does not denote anything,—e. g., "the present king of France,"—occurs in the statement of a proposition, the question as to the interpretation of propositions in whose verbal expression this phrase occurs arises, and Mr. Russell, in the article "On Denoting" referred to, succeeded in assigning a meaning to every proposition in whose verbal expression any denoting phrases—whether they appear to denote something or nothing at all, e. g., everything, nothing, something, a man, every man, no man, the father of Charles II, the present king of France—occur. It is not necessary to assume that denoting phrases ever have any meaning in themselves.

The theory of MacColl and the allied theory of Meinong were rejected by Mr. Russell[14] because they conflict with the law of contradiction. If any grammatically correct denoting phrase stands for an *object* although such objects may not *subsist*, such objects are apt to infringe the law of contradiction. Thus it is contended that the round square is round, and also not round.

To solve the paradoxes that appear in the mathematical theory of aggregates, Mr. Russell treated classes and relations in the same way as he treated denoting phrases.[15]

Poincaré, among others, recognized that all the paradoxes of the modern theory of aggregates, such as those of Burali-Forti, Russell and Richard, arise from a kind of vicious circle which may be expressed, in the language of Peano, thus: Everything which contains an apparent variable must not be one of the possible values of this variable.[16] But Poincaré did not perceive that if we wish to avoid such vicious circles we must have recourse to a fundamental re-moulding of logical principles, more or less analogous to the "no classes" theory. To have shown this seems to be one of Mr. Russell's greatest merits; simply because practically all the other mathematicians who have interested themselves in the paradoxes did not realize this important fact. Thus, said Mr. Russell,[17] the method by which Poincaré tried to avoid the vicious circle consists in saying that when we assert that "all propositions are true or false," which is the law of the excluded middle, we exclude tacitly the law of the excluded middle itself. The difficulty is to make this tacit exclusion legitimate without falling into the vicious circle. If we say, "All propositions are true or false, excepting the proposition that every proposition is true or false," we do not avoid the vicious circle. For this is a judgment bearing on all propositions, viz.: "All propositions are either true or false. or identical with the proposition that all propositions are true or false." And that supposes that we know the meaning of "all propositions are true or false," where *all* has no exception. That comes to defining the law of the excluded middle by: "All propositions with the exception of the law of the excluded middle are true or false," where the vicious circle is flagrant. We must, then, find a means to formulate the law of the excluded middle in such a way that it does not apply to itself.

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[12] "Ueber Sinn und Bedeutung," *Zeitschr. für Phil. und phil. Kritik*, Vol. C, 1892, pp. 25-50.

[13] *Mind*, N. S., Vol. XIV, 1905, p. 401.

[14] *Ibid.*, pp. 491, 482-483.

[15] "On Some Difficulties in the Theory of Transfinite Numbers and Order Types," *Proc. Lond. Math. Soc.* (2), Vol. IV, 1906, pp. 29-53 (cf. especially the part on the "No-Classes Theory"); "Les Paradoxes de la Logique," *Rev. de Métaphys. et de Morale*, Vol. XIV, 1906, pp. 627-650.

On the details of the new construction of logic in such a way that the paradoxes are avoided while nearly all of the work of Cantor on the transfinite is preserved, we must refer to Mr. Russell's works of 1908 and 1910.[18] Mr. Russell's method of avoiding the paradoxes in question is by what he called the "theory of types," and the object of this theory was shortly described by Dr. Whitehead and Mr. Russell[19] as follows: "The vicious circles in question arise from supposing that a collection of objects may contain members which can only be defined by means of the collection as a whole. Thus, for example, the collection of ***propositions*** will be supposed to contain a proposition stating that 'all propositions are either true or false.' It would seem, however, that such a statement could not be legitimate unless 'all propositions' referred to some already definite collection, which it cannot do if new propositions are created by statements about 'all propositions.' We shall, therefore, have to say that statements about 'all propositions' are meaningless. More generally, given any set of objects such that, if we suppose the set to have a total, then such a set cannot have a total. By saying that a set has 'no total,' we mean, primarily, that no significant statement can be made about 'all its members.' Propositions, as the above illustration shows, must be a set having no total. The same is true, as we shall shortly see, of propositional functions, even when these are restricted to such as can significantly have as argument a given object *a*. In such cases, it is necessary to break up our set into smaller sets, each of which is capable of a total. This is what the theory of types aims at effecting."[20]

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[16] We may also express this principle as follows: A collection of objects may not contain members which can only be defined by means of the collection as a whole.

[17] *Rev. de Métaphys. et de Morale*, Vol. XIV, pp. 644-645.

[18] "Mathematical Logic as Based on the Theory of Types," *Amer. Journ. of Math.*, Vol. XXX, 1908, pp. 222-262; A. N. Whitehead and B. Russell, *Principia Mathematica*, Vol. I, Cambridge, 1910, pp. 39-88.

[19] *Op. cit.*, p. 39.

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In the next place, we shall go back four or five years in time, and see how the distinction between *entity* and *existence* became necessary in a mathematical investigation which is somewhat familiar to me. If I consider, at rather greater length than it deserves, my own work of 1903 and 1904[21] on the contradiction of Burali-Forti and its bearings on the theory of well-ordered aggregates, it is merely because familiarity with this investigation enables me to point out a small, unobserved merit which it has, in distinguishing *entity* from *existence,* and also to give yet another illustration of the tendency—which seems particularly common with mathematicians—of holding to the belief in the being or existence or subsistence in some sense, of a non-entity.

Burali-Forti had found, in 1897, the now well-known contradiction arising from the fact that 'the ordinal type of the whole series of (finite and transfinite) ordinal numbers' appears both to be and not to be the greatest ordinal number. From this I concluded, in 1903, that there are no such things as "the type" and

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[20] The theory of logical types was described, in ordinary language, in *op. cit.*, pp. 39-68; and the theory of denoting was explained in the chapter on "Incomplete Symbols" (*ibid.*, pp 69-88).

[21] A general account of these investigations is contained in my paper, written in Peano's international (uninflected) Latin: "De Infinito in Mathematica," in ***Revista de Mathematica,*** Vol. VIII.

"the cardinal number" of the series just referred to. Hence, by a tacit use of an axiom afterwards stated explicitly by Zermelo, I concluded that every aggregate which has a cardinal number and every series which has a type can be well-ordered. The use of Zermelo's axiom was, with me as with most mathematicians, unrecognized; it occurred in some work of Mr. G. H. Hardy's on which I based my argument; and I was really concerned, not so much with the proof that every aggregate can be well-ordered, as with the proof that the series (W) of ordinal numbers has no type.

The matter becomes simpler to express when we consider *classes* instead of *series*. My contention, then, was that there is no such thing as "the cardinal number of the class of ordinal numbers" seems to represent. But if we adopt, as I adopted, the Frege-Russell definition of the cardinal number of a class u as the class of those classes which are similar to (can be put in a one-one correspondence with) u, there arises a difficulty. The cardinal number of the class w of ordinal numbers is the class of those classes which are similar to w; and this class certainly *exists*, for we can point out at least one member of it, namely, w itself, for w is similar to w. On the other hand, we have reason to deny that there *is* such a class as the cardinal number of w, and most mathematicians express this by saying that the cardinal number in question does not "exist." Of course, the solution of this apparent contradiction is that "the cardinal number of w" is a phrase denoting nothing—there is no such entity as the cardinal number of w. If it *did* denote a class, that class would be existent.

So, in my above-quoted paper, I distinguished between the existence of a class u from the entity of a thing v. The symbol "$\exists u$" was used, following Peano, to denote that u exists, and the symbol "Ev" was used to denote the proposition that v is an entity. The symbol "Ev" was defined by the definition of "not-Ev" as "v is a member of the null-class." Since the null-class has no members, and is defined as the x's satisfying a propositional function, such as x is not identical with x, which is always false, this is a most paradoxical way of stating the case about non-entity,[22] and the paradox results from the assumption that, in some sense, there *is* a v,—that, as MacColl would have said, v has a "symbolical existence." But, as Dr. Whitehead and Mr. Russell[23] pertinently remark: "We cannot first assume that there is a certain object, and then proceed to deny that there is such an object." Russell's solution of the difficulty about propositions asserting that "the so-and-so is not an entity" is to reduce all such propositions to a form not involving the assumption that "the so-and-so" is a grammatical subject. "The so-and-so," whether it appears to denote something or not, is an *incomplete* symbol, like the d/dx of mathematics.

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[22] On printing the above article, Professor Peano wrote to me, on Jan. 1, 1906, as follows: ".... I see the new symbol E, which you do not define symbolically, but the importance of which I believe I have understood....It would be necessary to introduce many kinds of null-class (Λ): $\Lambda_0 =$ that of the *Formulaire*; $\Lambda_1 =$ the class of classes, which has no classes; Λ_2 for the classes

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It has, I trust, been not quite without interest to see how the important distinction of *existence* and *entity* in mathematics struggled into clearness. We have seen before[24] that the discussions on "existence" of MM. Poincaré and Couturat were conducted in obscurity. This obscurity was produced by the confusion of the two notions of *existence* and *entity*, and the consequent use of one word to denote both.

When, in a paper published in 1904, I used the badly chosen term "inconsistent" for an aggregate whose cardinal number is a non-entity—"does not exist," I said then—Mr. Russell rightly objected that, given a class u, its cardinal number *must* exist, since u is a member of the class called the cardinal number of u. And yet there was an undoubted difficulty about what I called "inconsistent" classes. We know now that—at any rate when the number of a class is defined logically—it is a delusion that there are such "inconsistent" classes,—they are non-entities. If they *were* entities, their cardinal numbers would "exist."

There is one more thing to be noticed: it is the *entity* of a number that is most important, the proof of its *existence* is less so. In his *Principles* of 1903, Mr. Russell laid great stress on the existence-proofs of numbers and classes of spaces. Let us consider the case of real numbers. A real number is, according to Mr.

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of classes; Λ_3 for the classes of classes of classes;.... Λ_n,....Λ_ω,..... There is the generation of the transfinite numbers, in the principles of logic. There results this rather laughable consequence, that the new philosophers have decomposed *nothing* into a transfinite number of classes!"

[23] *Op. cit.*, p. 69. We may remark here, as I have done in a review of Whitehead and Russell's *Principia* in the *Cambridge Review* for 1911, that the authors (cf. pp. 32, 69, 182, 229) use the *word* "existence" ambiguously; though, of course, there is no ambiguity when the proper technical symbols ($\exists$ and E; E only occurring in a phrase involving incomplete symbols) are used.

[24] *Monist*, Jan. 1910, Vol. XX, pp. 113-116.

Russell, a certain class of rational numbers; its existence can be proved, and one feels satisfied. But a rational number or a negative number, being a relation, does not "exist," and yet one would have thought existence quite as important in these cases as in the case of real numbers.[25] I hope to go more fully into this question on another occasion.

PHILIP E. B. JOURDAIN.

CAMBRIDGE, ENGLAND.

IDEALISM AS A FORCE.

A MECHANICAL ANALOGY.

In the present state of knowledge the man of intelligence has much difficulty in deciding what course of conduct he should adopt in regard to beliefs and social and religious practice without at the same time violating these principles which he has obtained from science and critical philosophy. Before venturing to suggest exactly what position he should (and eventually must) take up, a little consideration of the importance of the older ideas and their relation to new ones would be advisable. I propose to introduce various mechanical analogies in this sketch, for two reasons. First, because I think they show forth more clearly the nature of the phenomena described, and second, a training in scientific thought soon shows one that mechanical laws pervade the whole universe, mental, moral and physical. I do not use the word "mechanical" in at all a derogatory sense. As a matter of fact, although it seems at first contrary to our ideas of perfection realized by a continuous process of adjustment, the really perfect state is the mechanical one, where each part has a definite and unchanging relation to all the other parts, so that a change in its condition is accompanied by a change in all other parts in accordance with the nature of that mutual relation. Surely this is what is meant by "correspondence with environment," if there is the proviso of stability. All moral philosophers have more or less directly stated that the key to morality is the Golden Rule, "Do as you would be done by," or as K'ung-fu-tze puts it, in one word, "Reciprocity," i. e., mutual bearing upon one another. This condition of mutual bearing is essentially, when complete, a mechanical state. Similarly in matters of thought consistency is the great principle, and what is consistency but a mechanically perfect state of balance? As to the mechanical character of physical conditions there can be no question, provided we do not necessarily limit the concept to the Newtonian exposition.

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[25] Frege (*Grundlagen der Arithmetik,* Breslau, 1884, pp. 114-115) indicated such definitions of all the numbers of analysis as would enable him to prove the existence in every case.

I wish to use frequently the idea of *force*. In natural philosophy a force is that which tends to produce or hinder motion, and it is the characteristic of all natural phenomena that the forces acting on them shall be in a state of *balance*. Whether they are still or moving, this balance exists either in the form of opposed pulls, pushes, stresses or accelerations of mass. It is the criterion in the light of which all mechanical problems may be attacked. I wish to extend this idea of force to matters of thought and ideal, by a definition such as the following: A mental force is that which produces or tends to produce change of thought.

The ever-famous Newton, in studying natural forces, announced three laws of motion. There is no definite proof of these, but we have no experience which contradicts them.

With the suggested psychical analogues these laws are as follows:

1. Any body tends to remain in its condition of rest or motion until acted on by some force.

To extend this to matters of thought we can say:

Any idea (group of concepts) tends to remain in its state of rest or change along certain lines until acted upon by some mental force.

2. Change of motion is proportional to the magnitude of the applied force.

This becomes:

Change of thought is greater or less according to the effective importance of the mental force.

3. To every action there is a reaction, i. e., whenever a force acts upon a body there is called out in that body a force opposed to (and equal to) the first force which manifests itself as internal stress or acceleration of mass.

In mental matters this notion is expressed by the change in thought which takes place as the result of applying mental force, appearing either as a new formation of ideas or a reaction of old ideas on the new mental force.

It must be understood at this point that I do not mean anything extremely mystical or undiscovered by this term "mental force." I simply give this name to a set of ideas, in the first place external to the mind in question, then received through the ordinary channels of sense, and acting upon the ideas already existing there, either producing resistance or modifying those ideas. The technical word "suggestion" is almost identical in meaning.

The engineer, in the spirit of Newton, takes our above-described three laws into one equivalent, as follows:

Force is the rate of change of motion attached to matter (technically "momentum").

This simply means that wherever and whenever a force acts upon a body it produces a change in its motion, or, *vice versa,* a change in motion is caused by a force.

This can be made the basis of a more sweeping statement which describes mental force thus:

Mental force is the rate of change of thought attached to mind. (Brain-matter is perhaps not to be regarded as the absolute medium of thought, since psychologists regard the latter as contemporaneous with, but not necessarily the same as, change in cerebral substance).

Idealism I wish to describe as a particular type of mental force proceeding in the first place from some external source, and then by its action on different minds in accordance with the above laws and by the reactions of such minds on physical and moral actions, producing an effect tending to the realization of certain progressive states which are for the time being regarded as perfect.

In the light of this conception all religions are forms of idealism.

If we examine any religion from its commencement we usually find some such development as this:

1. Absorption by a master mind (the founder) of certain older ideals, the mutual reactions of which together with the mental contition induced in him by his surroundings (physical and social) produce a new system with one central ideal.

2. This result in many cases is accompanied by very severe mental strain, and in some cases by nervous disease (cf. Mohammed who is believed to have suffered from epilepsy) after which this ideal takes the leading part in his thought and life (monoideism).

3. The ideal now works through him to the minds of certain followers or disciples who receive it according to their previous

training and heredity, and so is formed a circle of minds in which the ideal circulates for a time, gaining an ever increasing potential.

4. The widening of the circle and frequently the loss by decease of the founder, causes the ideal to cease its original evolution and take on certain new features according to the reactions in the minds of its various adherents. Hence we have lesser circles forming, to which certain new phases have more and more relation, until there is a schism of the original community and the most energetic minds found sects.

5. These sections expand or not according as the ideal is resisted or absorbed by the further minds upon which it acts, and we may finally have a large community with the ideal (usually much modified by reaction) controlling and connecting the units. This arrangement persists until external ideas of a different kind or internal resistances destroy its energy and it is replaced by other ideals or a great modification of the old one.

The mechanical analogy to the action of external forces on matter already possessing kinetic energy is so obvious if the lines previously indicated are followed, that I will not trace out each link of the chain, but merely point out the steps in which we draw a comparison.

1. Composition (i. e., combining together) of various forces (ideals) in one point (mind) which possesses considerable freedom (enthusiasm).

2. Acceleration in this point (mind) under the resultant force (new ideal) finally acting on other bodies (minds) in a greater or less degree according to their condition of stability (environment).

3. Composition of the forces in these individual bodies (minds) resulting in a balanced but unstable system (idealist community).

4. Splitting up of systems into smaller systems (sects) balanced in themselves with moderately high stability (sects) and balanced as a whole (unstably) as a general system (national religion).

5. Modification of system by new forces (ideals) finally resulting in a new system (religion).

At this point it is necessary to discuss the importance of idealism in its effect on the social life. Once a definite ideal or system of ideals has become established among a set of minds it acts as a "superhuman" power (not in the accepted sense of "supernatural" but as the simple result of evolution) whose magnitude is the resultant of the various forces which it has impressed on individual minds and whose direction (i. e., tendency to progress or degenerate) is determined by the manner in which it has combined with the mental forces previously impressed on these minds.

We see then that it has a definite (but fluctuating) value, a more or less constant direction (for the time) and it is attached to a certain number of unit minds.

It may be compared with the constitution of the atom in which there are a number of electrons each possessing a peculiar resultant motion of its own but at the same time coordinating with other electrons to confer on the atom as a whole certain dynamic properties which manifest themselves as polarity or chemical attraction, which, although the equivalent of the electronic energy, are different in kind.

Similarly our ideal may be attached to a large number of minds of varying caliber, force and direction, but as a whole organism the system will be possessed of properties differing from those of its units.

Such a force as this centered in a community constitutes a divine being controlling and working through its members, just as according to modern psychology, the soul is a centering of nervous energy. The Christian church in which the members are said to belong to the mystical body of Christ exemplifies this. The whole of the church is, so long as homogeneity prevails, a force whose magnitude is the resultant of the mental and moral efforts of the units. These efforts may be distinct in kind, amount and object, but nevertheless on the whole they are cumulative and there is a resultant which may be well called the living Christ, for it is an intelligent force realizing within itself to some extent the ideal which the master-mind of Jesus impressed on his disciples to such a degree as their capacities permitted.

In this way the doctrines of salvation (i. e., separation from anti-Christian community and ideals) and grace (impression of idealism according to capacity for receiving it) become explicable and even reasonable. Of this more later.

I am of course aware that I at once lay myself open to severe criticism from the adherents of all faiths who conceive their deity to be omnipotent and omniscient. To this notion I would say that such a force as described above has within itself the means of doing and knowing all those things which come within the ken of the units, and that further it combines with the resultant forces of the universe, being either decreased or increased in effect according as it is opposed to or in line with such world forces. So long as a religion progresses (apart from the consideration of certain artificial conditions such as politics) it must be to some extent in conformity with the laws of the universe, known and unknown. So soon as it directly opposes those laws (still subject however to certain sociological factors) it must degenerate. The gods of a religion live and die with it, their energy appearing in other faiths after reaction has taken place in the minds of the interregnum. The only case in which they (or he) are immortal is when they are definitely identified with some permanent force in the universe so that the mental force runs contemporaneously with a natural one, each producing proportionate effects on mind and matter. It is from this cause that Judaism has ensured its immortality. About the time of the Captivity it definitely connected its tribal deity Yahweh not only with the ideal of *tzedek* (righteousness) but with that unitary world-power which under various names (such as "the eternal energy") all philosophers and scientists recognize, with or without moral attributes. This element of permanence has been transmitted to Christianity and Islam so that these three are probably the most stable of all faiths. It does not however necessarily follow that because the force survives, the attachment of the community to the ideal force will also survive. Its energy may be transferred to other minds, possibly in other forms, but practically never losing all connection with the primal natural force with which it has been associated.

In order that the idealism of a community shall have a permanent effect it is necessary:

1. That there should be a continual supply of mental energy on the part of unit minds;

2. That the individual energies shall be so directed generally and of such amount that there always is an external resultant producing progress by its reaction on the minds of both the units of the community and those outside of the community.

In order to assure the first condition some definite "cult" is required, which by the repetition of various practices concentrates the mind on the ideal tending to develop its realization in that mind and directing the energy of the mind to that end, both within and without.

In the second condition it is essential that certain agreements concerning the ideal shall be established, so that the energies put forth are not contrary in tendency. This is the foundation of dogma, which states as far as possible the ideal in words and symbols, which produce in the various minds a more or less homogeneous conception of the ideal.

Further, it is necessary in order that the mental forces shall not equilibriate, that all the members of the community shall, as far as practicable within the limits of the competition necessitated by the law of selection and survival, support one another, so that the mutual stress between them is minimized and the external resultant increased.

To return to our electron analogy, if electrons move at right angles to the general path, collisions will occur which reduce the external force exerted by the atom, and if sufficiently numerous may be conceived quite to destroy that force and even disintegrate the atom. (Cf. "The house divided against itself.")

This necessity for internal balance gives rise to ethics, which is summarized by the Golden Rule.

HERBERT CHATLEY.
CHINESE GOVERNMENT ENGINEERING COLLEGE.
TÂNG SHAN, CHIH-LI.

CLASSICAL CONFUCIANISM.

Sinology has so far not yet passed the stage of crude and amateurish translation. No interpretative work worthy of serious consideration has yet appeared. Mr. Miles Menander Dawson's recently published book, *The Ethics of Confucius: The Sayings of the Master and his Disciples upon the Conduct of the "Superior Man,"*[1] is an attempt in the direction of interpreting Confucianism to the West. We congratulate him on his highly successful exposition of one of the greatest ethical systems of the world. His work has at least met a need which has long been felt by all who desire to bring about a better understanding of Chinese civilization in the occidental world. For ever since the days of Marshman and Legge the true meaning of Confucianism has been lying hidden in those painstaking but unfortunately too expensive and out-of-print translations; and the general public have long had to swallow what superficial and biased writers are pleased to call "Confucianism." Mr. Dawson's book is based entirely on Legge's translation of *The*

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[1] New York, G. P. Putnam's Sons. Pp. xviii, 305. Price, $1.50 net.

Chinese Classics, and he has so classified and arranged his material that the reader can easily comprehend what Confucius and the early Confucians actually said on the various fundamental problems of life.

This book has many notable merits. First, the handling of the immense quantity of material is excellent. The work is divided into seven chapters: I. What Constitutes the Superior Man; II. Self-Development; III. General Human Relations; IV. The family; V. The State; VI. Cultivation of the Fine Arts; VII. Universal Relations. Mr. Dawson has seized upon a very important point in Confucianism when he arranges his book in accordance with the scheme of *The Great Learning*. For the Confucian ethics is essentially a system of human relations: all extension of knowledge contributes to the cultivation of individual conduct, and from the individual there radiate the relationships of the family, the state and the world.

Secondly, the illustrative quotations from the Confucian classics are, with a few exceptions, very well chosen. The quotations are all accompanied by the name of the book, the number of chapter, paragraph and verse. The carefulness and patience with which the numerous passages are selected and classified, certainly commands our admiration. The index appended to the book also enhances its usefulness.

Thirdly, the first two chapters in particular constitute the best portion of the book. In these chapters Mr. Dawson sets forth the Confucian ideal man, "the Superior Man," which forms the subtitle of the book. The Superior Man, which can be more literally translated as "the lordly man" or better still as "the gentleman," is quite different from the dianoetic man of the Greeks; neither does he aspire to the Nirvanic life of Buddhism, nor aim at the attainment of a union with God, which forms the ideal of Christianity. The Confucian ideal is simply a life made ever nobler and richer by individual reticence and by a conscious adoption as one's own of the social moral institutions which constitute the *li* (translated "rules of propriety") or what the Hegelians call *Sittlichkeit*. In expounding these basic elements of Confucianism Mr. Dawson has exhibited a high degree of clarity of exposition and richness of illustration.

Lastly, we believe that the greatest merit of the book lies in its objectivity, by which is meant the impartiality and disinterestedness with which the author expounds the Confucian doctrines.

Mr. Dawson has no desire to prove that Confucianism is inferior to any particular ethical or religious system, nor does he wish to proselyte his readers into Confucianism. He simply presents to us what the great Confucians thought and taught concerning the multifarious complexities of life and conduct. He speaks of concubinage with the same calmness with which he discusses the Confucian conception of the state.

It is natural that an undertaking of this kind by one who has no access to the original texts cannot be entirely free from occasional errors. Numerous unimportant mistakes may be pointed out at random. For example: (1) on page xiii, the name of Confucius appears twice as *Kung Chin*, which should be *Kung Chiu*; (2) on page xiv, *Chun Chin* should read *Chun Chiu*; (3) on page xvi, it is wrong to include the *Hsiao King* instead of the *Chun Chiu* in the Five Classics; and (4) on the same page "Pan Ku" and *The History of Han Dynasty* are mentioned as two separate works; whereas, as a matter of fact, Pan Ku is the author of *The History of Han Dynasty*.

Of errors of a more serious nature we find at least three. In the first place, the title, "The Ethics of Confucius," is not correct. It is as if a compilation of the ethical theories contained in the works of Plato, Aristotle and Theophrastus were to be called "The Ethics of Socrates." Mr. Dawson's book deals with the ethics, not of Confucius alone, but of what we may call classical Confucianism. For it is almost needless to point out that many of the Confucian classics, like the *Shu King* and the *Shi King*, deal with historical periods long before Confucius; while others, like the *Book of Mencius* and the *Li Ki*, came long after the death of Confucius. Book III of the *Li Ki*, for example, was compiled in the second century B. C.

In the second place, Mr. Dawson has at times misinterpreted the meaning of certain passages. Take this illustration:

> "The scholar keeps himself free from all stain" (*Li Ki*, xxxviii, 15). The Master said, "Refusing to surrender their wills or to submit to any taint to their persons; such, I think, were Pih-E and Shuh-Tse" (*Analects*, xviii, 8).

"These two passages," says Mr. Dawson, "illustrate the sage's insistence upon sexual continence, among other virtues." Now the word "stain" in the first quotation has no reference to sexual relations. Nor does the phrase "taint to their persons" in the second quotation mean sexual immorality. The story of Pih-E and Shuh-Tse (or Po-I and Shu-Chi), who abandoned their hereditary kingdom and retired into obscurity, and who, when the Chou Dynasty was founded, died of hunger rather than live under the new dynasty,—this story is well known to every Chinese, and is given in a note in Legge's translation (v. 22).

In the third place, Mr. Dawson has on several occasions taken a passage quite apart from its immediate and inseparable context, thus losing the meaning that was intended. An example of this kind is found on page 248:

> "When good government prevails in the empire, ceremonies, music and punitive military expeditions proceed from the emperor" (*Analects*, xvi, 2).

This passage Mr. Dawson takes as "suggesting that wise patronage and encouragement of art by the government which has distinguished the most enlightened governments of ancient and modern times." Now this passage cannot be taken apart from its context. Here is the context:

> "When good government prevails in the empire, ceremonies, music, and punitive military expeditions proceed from the emperor. When bad government prevails, these things proceed from the princes. When these things proceed from the princes, rarely can the empire maintain itself more than ten generations."[2]

Here we can easily see that the point of emphasis in this passage is from what source these institutions should derive their authority. The passage no more illustrates the wise patronage of art than it illustrates the encouragement of punitive expeditions.

It must be pointed out, however, that such errors are very rare in the entire work. On the whole, Mr. Dawson's book may be recommended to all students of Chinese philosophy and religion as an excellent exposition of classical Confucianism.

SUH HU.

COLUMBIA UNIVERSITY, New York.

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[2] This is my translation. Legge's rendering is not correct.

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THE MONIST

THE TEXT OF THE RESURRECTION IN MARK, AND ITS TESTIMONY TO THE APPARITIONAL THEORY.

WITH A PREFACE ON LUKE'S MUTILATION OF MARK.

THE greatest literary problem in the New Testament is: What is the matter with the Gospel of Mark? Something happened to the end of it in the first or second century, and for ages thereafter it was left truncated in the middle of a sentence or else supplied with a shorter conclusion than the present one, which scholars long kept to themselves. Edwin A. Abbott, however, gave it in his forgotten Gospel analysis of 1884, and the Nonconformist translators of *The Twentieth Century New Testament* have also given it; but it does not appear in any official translation, though the Revised Version mentions it in a note at Mark xvi. 8. This is the note:

"The two oldest Greek manuscripts, and some other authorities, omit from verse 9 to the end. *Some other authorities have a different ending to the Gospel.*"

Here is the "different ending," translated from a ninth-century manuscript in the National Library of France, Codex L, which gives both conclusions, but puts this one first. (We prefix to it the connecting words of Mark):

II. *And they went out and fled from the sepulcher, for* *trembling and astonishment had come upon them; and they said nothing to any one, for they were afraid of.*

* * * * * * * * * * * * *

[Thirteen ornamental marks.]

Where also you must give currency to this:

Now, all things that were commanded, they showed forth in few words unto those about Peter. And after these things Jesus himself, also, from the East even unto the West, sent forth through them the holy and incorruptible preaching of eternal salvation.

But there is also current the following, after the words: FOR THEY WERE AFRAID OF:

Now, when he was risen early etc. (as in our common versions, Mark xvi. 9-20).

In their Introduction to the New Testament (Cambridge, 1881, pp. 298, 299) Westcott and Hort remark on the above "less known alternative supplement" to Mark: "In style it is unlike the ordinary narrative of the Evangelists, but comparable to the four introductory verses of St. Luke's Gospel." Conybeare, in his great book, *Myth, Magic and Morals,* throws out the suggestion that Luke mutilated the first edition of Mark because he disagreed with its contents: viz., an account of apparitions in Galilee, whereas he expressly limits all these phenomena to Judea, by making Jesus order the apostles to stay in Jerusalem until Pentecost. (Luke xxiv. 49; Acts i. 4). If Luke mutilated Mark, then why not go further and say that he wrote this smooth-flowing supplement to round him out? The word συντομως, "in few words," is never found in the New Testament except in this shorter Mark Appendix and in Luke's Acts of the Apostles (xxiv. 4).

'Εξαγγελλω, "to show forth," also occurs only in the Pauline or Lucan (for Luke was Paul's secretary) Epistle of Peter (1 Pet. ii. 9). 'Εξαποστελλω, "to send forth," is used seven times in Acts, thrice in Luke's Gospel, and once by his master Paul. "Incorruptible" occurs only in Paul and the Pauline 1 Peter.

Luke represents the aristocratic tradition of the capital, which said: "It all happened here!" Mark represents the rural tradition of Galilee, which said: "Our poor parish was the scene of these wonderful things!" So the young man in white, in Mark, says at the sepulcher: *Go, tell his disciples and Peter: Behold, I am going to Galilee ahead of you. There shall ye see me.* (Thus read some of our best manuscripts, in the first person.)

Another thing: Luke and John both make the apparitions real. In these later Gospels Jesus is objective after the Resurrection: he eats broiled fish in Luke, while in John the wounds in his hands and side are felt by Thomas. Now as the earliest account of the Resurrection in Paul (1 Cor. xv. 4-8) makes the event a series of apparitions, it is probable that the second earliest account, Mark's, did the same. Indeed in Matthew xxviii. 17 (under suspicion of being taken from the lost ending of Mark), "some doubted." This was because some saw the figure and others did not. Luke and John leave no room for doubt: the evidence is sensuous, not subjective.

The first Christian heresy was Docetism, the belief that Jesus even in life was a phantom. His flesh and blood were unreal; he did not really suffer; his bodily functions were different from human ones or even non-existent. To fight this heresy the First Epistle of John was written, and a curse pronounced upon those who doubted that Jesus had been actual flesh and blood (1 John iv. 2, 3). Consequently if Mark repeated Paul's impression that the Galilean apparitions were the same in kind as the one to himself on the Damascus road, then Mark must go. Who was the likeliest one to do this work of excision? Answer: Luke. He was the most literary of all the Evangelists. He is the only one of them who says "I." Moreover, as Harnack has pointed out, he betrays an animus against Mark, animadverting upon his conduct in Acts xv. 36-41. In his own Gospel Prologue, Luke is undoubtedly thinking of him as one of the "many" who have "undertaken" to write the life of Jesus, but who have not begun "accurately from the first" nor set forth "in order" the sayings and events. Add to this the Jerusalem tradition of the Resurrection against the Galilean, and the flesh-and-blood appearances against the phantom who is only to be "seen" ("there shall ye see me," in Mark), and we have motive enough for Luke's high-handed act.

Indeed, we can even surmise the reason why he made the excision in the middle of a sentence. He would hardly do this except to get rid of an offensive word. If Mark had read:

They said nothing to any one, for they were afraid of the apparition,

this last word would have been the red rag. There must be no apparition: there must be objective forms. The young man in white, who, in several MSS., speaks in the person of Jesus, was indeed he himself in his glorified being. Thus do I read the texts. Luke too had read something of this kind, which he reproduces thus:

But they were terrified and affrighted and supposed that they beheld a spirit. (Luke xxiv. 37. The Cambridge MS. and Marcion's edition of Luke both read "apparition" instead of "spirit.")

Let it be understood that I do not deny the possibility of ectoplastic phantoms, which Myers himself believed in, though he said he would not press them upon the credence of the reader, because of the difficulty of correct observation and the chances of fraud. Dr. Reichel of Germany has testified to their occurrence here in America. The difficulty in the New Testament is that they only appear in the later accounts. Paul and (I shall show presently) Mark, our earliest witnesses, know of apparitions alone, not of materialized forms.

For the fullest account in English of all the problems the reader should consult *The Resurrection in the New Testament,* by Clayton R. Bowen, of Meadville, Pennsylvania (New York, 1911). Professor Bowen is one of a long series of laymen and liberals, like Griesbach, Lachmann, Tischendorf and Tregelles, who have taken the New Testament out of clerical hands. The three German lay professors and Tregelles, the English Quaker, were the ones whose work led directly to the Revised Version of 1881; but the task of revision is by no means ended yet.

Bowen was a Unitarian minister, but is now professor at Meadville. Before reading him, a shorter and clearer book by Kirsopp Lake should first be mastered.

* * *

Kirsopp Lake, of Harvard University, published in 1907 *The Historical Evidence for the Resurrection of Jesus Christ* (London, Williams and Norgate). Professor Lake at that time held the chair of New Testament Exegesis in the University of Leiden, to which Rendel Harris was elected in 1903, but did not serve. The book appeared in the Crown Theological Library and has been widely read. It contains a masterly analysis of the Resurrection narratives in 1 Corinthians, the Synoptical Gospels, the Acts of the Apostles, the Mark appendices, the Fourth Gospel and the apocryphal ones of Peter and the Hebrews. The conclusion reached is that Paul and Mark's accounts are historical, and the later ones exaggerated. Babylonian and other resurrection theories are reviewed, and the book ends with an allusion to Myers and psychical research. F. C. Burkitt, of Cambridge, in placing the essay in a bibliography, says: "I introduce this book here as the first example in original English work of the doctrine of the priority of Mark being consistently applied throughout an historical investigation." (*The Earliest Sources for the Life of Jesus.* Boston, 1910, p. 129).

The method is that of the Lower Criticism, though the Higher is also freely used. What I especially wish to criticize is the following passage (pp. 61-65) which here we must read in full:

"*The young man at the tomb.*—The account of what the women saw at the tomb is contained in Mark xvi. 5. Dependent narratives are found in Matthew xxviii. 2-5 and in Luke xxiv. 3-5.

"*And entering into the tomb, they saw a young man sitting on the right side, clothed in a white garment; and they were astonished.*

"As it stands in Mark, this account gives rise at once to two questions: Did they see for themselves that the grave was empty? and who was the young man who appeared to them? Neither question is answered in Mark, but before considering the bearing of this fact, it is first necessary to ask whether the version given above represents the original text. According to it, the women entered the tomb and found a young man seated within on the right hand. No other meaning can be extracted from it, or ever could have been, in the presence of the word εἰσελθουσαι, 'entering into,' in verse 5 and the reference contained in the corresponding ἐξελθουσαι, 'going out,' in verse 8. But in case of neither of these words is the text perfectly certain. The former is in the Vatican MS. weakened to ἐλθουσαι, 'coming,' while the latter is not represented in the Arabic Diatessaron, and in some MSS. is altered to ἀκουσαντες, 'having heard.' The weight of textual evidence is against these alterations, but on the other hand transcriptional probability is in their favor. It is unlikely that later scribes would have introduced changes in the text which were calculated to weaken the evidence for the belief that the women had made a complete examination of the tomb, and if these changes be made, the text of Mark would leave it doubtful whether the women saw the young man on the right hand of the inside or of the outside of the tomb; for ἐλθουσαι εἰς το μνημειον need not mean more than 'when they came to the tomb.' Is it possible that this represents the original form of the narrative? In the absence of other evidence, it may not be ill-advised to consider the evidence of a comparison with the two other gospels, Matthew and Luke, which are closely based on the Marcan narrative, and of the Fourth Gospel and the Gospel of Peter, which follow it with greater freedom. It has already been seen, in cases in which the Marcan document is undoubtedly ambiguous or difficult, that the dependent narratives adopted divergent methods of elucidating the points at issue. It may therefore be allowed to reverse this argument and see whether the dependent narratives in the present case support the suggestion that the ground document was ambiguous. They certainly seem to do so. Matthew represents the angel, who is in his narrative the equivalent of the young man of Mark, as seated on the stone which he had just rolled away; he was therefore regarded by Matthew as *outside* the tomb. It is equally plain that Luke regards the two men, who in his narrative represent the Marcan young man, as appearing *within* the tomb. Furthermore, the Fourth Gospel and the Gospel of Peter narrate that the women did not enter the tomb, but stooped down and saw an angel or angels sitting within. These two last accounts may quite well represent an attempt at conflation between two traditions which differed, or were not explicit, as to the position of the women and the angel with regard to the tomb, and so far they support the suggestion, which is rather strongly made by Matthew and

Luke, that the ground document was ambiguous on this point. The weak point in this argument is that it does not take account of the possibility that Matthew altered the Marcan document owing to the influence of the story of the watchers. It could be argued that the angel had to be kept in the presence of the watchers and of the women, and that the word ἀπελθοῦσαι, 'going from,' in verse 8 is a proof that the ground document of Matthew contained an account of an actual entry into the tomb. This is perhaps not a convincing argument, but it may be taken as practically balancing the previous one. It is impossible finally to decide between the two. I think that the balance of probability remains slightly in favor of the view that the original Marcan document narrated the story of the vision at the tomb in such a way, as not to state plainly that the women entered the tomb, but I should not be prepared to put emphasis on the argument."

I hope to show that there is every reason for Professor Lake to emphasize the argument that the original text of Mark did actually keep the women outside the tomb. We may say *does actually,* for the original text of Mark can be reconstructed from extant manuscripts and versions, without any appeal to the Higher Criticism. In one case only do we have to appeal to a lost source, but even this is supported by a patristic quotation, and therefore belongs to the Lower Criticism.

Let us begin with this lost source. Eusebius, in his *Questions of Marinus,* Question 1, which deals with the absence of the Mark Appendix (Mark xvi. 9-20) from the oldest manuscripts, says this:

"He who rejects the passage itself might say that the story does not exist in all the copies of the Gospel according to Mark; at least, the accurate ones among the copies describe the end of the story according to Mark in the words of the youth who appears to the women, saying to them: *'Be not astonished; ye seek Jesus the Nazarene,'* and so forth. *'And when they heard, they fled, and said nothing to anyone, for they were afraid of....'* For herein the end is described in nearly all the copies of the Gospel according to Mark, and what follows is seldom found in any, but would not be superfluous in all, and especially if they should contain a contradiction to the witness of the rest of the Evangelists."

We may remark that "afraid of" is Kirsopp Lake's own translation of the concluding words of the genuine text of Mark, and it has been adopted by James Moffatt in his splendid translation of the New Testament (London, 1913). But the words for which we have copied this famous passage of Eusebius are: "when they heard" (ἀκουσασαι). Now it is known that Eusebius had access to the library collected by Origen in the third century and extended by Pamphilus. Indeed Conybeare has made use of this fact to delete the trinitarian formula and the baptismal charge at the end of the Gospel of Matthew, in the teeth of all existing manuscripts. He shows that Eusebius read Matthew xxviii. 18-20 without these theological additions, and places over against three thousand extant MSS., all later than the fourth century, that other thousand, now lost, which went back to the third and the second.

Applying this principle we can put in the forefront of our textual evidence for ἀκουσασαι instead of ἐξελθουσαι the whole weight of the earliest Christian manuscripts. The ungrammatical ἀκουσαντες quoted by Lake is from a medieval manuscript in Russia, numbered 565 by Caspar René Gregory in his Prolegomena to Tischendorf's Greek Testament. Of course Eusebius gives the right reading, ἀκουσασαι (feminine). Rallying to the support of this ancient Greek original are the Washington manuscript and the Old Syriac and Old Armenian versions, overlooked by Lake. Their testimony is very important; especially the Armenian, for the Old Syriac and the Washington Greek betray a transition stage which was tautological. The "went out" was evidently interpolated before the deletion of the "having heard."

The following table will give a view of the process of corruption. As Eusebius expressly tells us that the most accurate MSS. omitted the Mark appendix, we need only deal with those that do so. This gives us a sure criterion. Six MSS. that omit this can therefore be pitted against 6000 that add it. To the trustworthy ones we may add those which contain the spurious matter with a caveat, also those which have a different ending from the current appendix. To these also must be added a few MSS. that contain attestations of careful copying from Jerusalem copies, such as No. 565.

Lost MSS. of the Early Centuries quoted by Eusebius.

[First clause not traced.]

And when they heard they fled and said nothing to any one, for they were afraid of......

(*End of Mark.*)

Armenian Version.

And entering into the sepulcher.

* * *

And when they heard, they fled from the sepulcher, because they were terrified; and they said nothing to any one for they were afraid.

Gospel according to Mark.[1]

* * *

Introduction to Luke.

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[1] The colophons here printed in bold-faced type are rubricated in the original.

Frank Normart, of Glenolden, Pennsylvania, but a native of Erzerum, has translated for me the passage from the Old Armenian, as found in his own printed edition (Constantinople, 1895) and in a valuable manuscript owned by John P. Peters (Bedrosian) of Philadelphia. (The colophon is from the manuscript, for the Bible Society has printed the Appendix, as in the King James version, with a note accusing the Greeks for omitting it, but carefully suppressing the fact that nearly all Armenian MSS. before A. D. 1100 omit it also.)

Both in the Syriac and the Armenian this colophon is rubricated.

Washington MS.

And entering into the sepulcher

* * *

And when they heard, they went out and fled from the sepulcher, for fear and astonishment had come upon them, and they said nothing to any one, for they were afraid of. Now when he was risen early, on the first day of the week, he appeared to Mary Magdalene etc.

> [This is the earliest MS. that contains the Appendix, which it has in an unusual form, hitherto only partially known from a fragment in Jerome.]

Old Syriac.

And they entered into the sepulcher

* * *

And when they heard, they came forth and went away and said nothing to any one, for they had been afraid.

ENDETH GOSPEL OF MARK.

The South Coptic
(Sahidic or Thebaic.)

[First clause wanting.]

* * *

And when they had heard, they came out of the sepulcher, and they ran, for a trembling was laying hold on them, and a confusion; and they said not any word to any one, for they were fearing. But all the things which were ordered them, to those who followed Peter they said them openly. After these things also again Jesus was manifested to them from the place of rising of the sun unto the place of setting. He sent through them the preaching which is holy and incorruptible of the eternal salvation. Amen.

But these also belong to them.

[Then follows the Longer Appendix, after a repetition of the words at the juncture.]

The Vatican MS.

And coming unto the sepulcher

* * *

And they went out and fled from the sepulcher, for trembling and astonishment had come upon them; and they said nothing to any one, for they were afraid of......

ACCORDING TO MARK.

The Sinaitic MS.

And entering into the sepulcher

* * *

And they went out and fled from the sepulcher, for trembling and astonishment had come upon them; and they said nothing to any one, for they were afraid of......

Gospel according to Mark.

The Old Latin at Turin.

And when they had entered, they saw a youth etc.

* * *

But when they went out from the sepulcher, they fled; for trembling held them, and awe by reason of fear.

But all things whatsoever that were commanded, those also who were with the boy briefly explained. And after these things Jesu himself appeared, and from the East even unto the East (*sic*) he sent through them the holy and uncorrupted [preaching] of eternal salvation. Amen.

Endeth Gospel according to Mark. Beginneth happily according to Matthew.

The Ethiopic version also omits the Mark appendix, while medieval MSS. L, and Nos. 1 and 209 show the doubt about it; L gives both endings, like the South Coptic, putting the shorter appendix first as we have seen already. Nos. 1 and 209 say at xvi. 8:

"In some copies, the Evangelist ends here, as far as Eusebius the [friend] of Pamphilus, has placed his canons; in others, there are found also these [words]:

"NOW WHEN HE WAS RISEN," etc.

Several Fathers support the testimony of Eusebius, so that the proof is overwhelming.

By restoring "when they heard" at xvi. 8, we get rid of the ἐξελθουσαι, but this was introduced as correlative to εἰσελθουσαι: the two stand or fall together. If we had not a single manuscript that read ἐλθουσαι at xvi. 5, the Higher Criticism would bid us read it. But our very oldest Greek authority reads it, plus an eleventh-century MS numbered 127, together with the fourth-century Gothic version. This reads *atgaggandeins*, "coming at," or "going unto." Lower Criticism therefore permits the restoration.

So we have unimpeachable ancient testimony that there was no ἐξελθουσαι at Mark xvi. 8: the women did not flee out of the tomb, because they had never been in it.

It is vain to protest that we cannot put three authorities against three thousand that read εἰσελθουσαι, "entering into"; for, by the laws of the Lower Criticism, we can, not only on the grounds already given, but by a well-known law of textual criticism. Dean Alford, in the critical apparatus to his Greek Testament, gives us the reason:

"Received text, εἰσελθουσαι, *from the parallel in Luke.*" (Henry Alford, *Greek Testament*: New York, 1859, Vol. I, p. 391.)

Nay, more: the Dean of Canterbury Cathedral did not hesitate to put ἐλθουσαι into his Greek text and to translate it in his *New Testament for English Readers* (London, 1868, Vol. I):

"And when they came to the sepulcher," etc.

The principle upon which Alford did this is perfectly sound. It is thus expressed by Jerome in his letter to Pope Damasus, introducing his novel Vulgate edition of the Gospels in the year 384:

"Great error, if indeed it be (so), has grown up in our codices, so long as what one Evangelist has said further on the same thing, (the scribes), have added in another because they thought it too little. Or, so long as another expressed otherwise the same meaning, he who had read first any one of the Four, considered that the rest ought also to be amended to the pattern of that one. Whence it happens that among us everything is mixed, and *there are found in Mark more things of Luke and of Matthew,* and again in Matthew more things of John and of Mark, and in others of the rest things which are peculiar to others."

Here we have the Protestant reason, stated by the prince of Catholics, for modern revision of the text. Although we have not access to so many ancient manuscripts as Jerome had, yet we have ancient versions neglected by him, as well as two Greek codices of his own time and several Latin ones. We are more justified in ceasing to regard his work as final because he tells us himself that he did not do it thoroughly:

"This short preface offers only the four gospels, the order of which is as follows: Mathew, Mark, Luke, John, amended by a collation of the Greek codices, but (only) of old ones. Lest, however, they should differ much from the accustomed form of the Latin reading, we have so restrained the pen that, when such things only were corrected as seemed to change the sense, we suffered the rest to remain as they were."

We are now in a position to reconstruct the end of the Gospel of Mark, and to show that this most historic of all the Evangelists never told a story about a corpse that got up and walked off, but simply of some women who came to a tomb and saw a strange young man. When they saw him they were astonished, but when he addressed them they were terrified and ran away. At this point the Gospel ends, as we now have it. The reason for this abrupt ending requires a separate discussion, such as briefly outlined in our prologue or such as I attempted in *The Open Court* at Easter, 1910. Unfortunately I had not then read Kirsopp Lake, or my attempt would have been better. (At the top of page 133 I made a blunder; line 1 should read: "Now this note of doubt is Marcan, not Matthaean," etc.)

In giving the following text, several readings differing from the common ones have also been given in addition to those noted. Thus the phrase "on their right" in verse 5 is from the Sinai Syriac, and is all part of the reformed readings, for it no longer smacks of the inside of the grave where a youth or an angel was sitting on the right side of the corpse. The word "daughter" is also from the Old Syriac. The Greek has a blank here, which our common translations supply by "mother." But a second-century version in the language which Jesus spoke may be presumed to go back to authentic tradition.

Our reconstruction is guided by the Lower Criticism alone. If we were to venture upon the dangerous ground of the Higher, I should strike out the words: "He is risen; he is not here." These are lacking in important MSS. of Luke, and as Luke used the first edition of Mark, he probably did not find them therein. But, as they appear in all extant MSS. of Mark, the principles of the Lower Criticism require that they should be retained. Higher Criticism would also query the historicity of the spices and ointment. Matthew says nothing about them, but tells us that the women came "to see the tomb," and had no errand inside. John expressly rules out the proceeding by an elaborate embalming before burial. If Luke's εἰσελϑουσαι could be copied into Mark, as Alford following the statement of Jerome and the abundant witness of the manuscripts would have us admit, why should not Luke's ointment and spices also have found their way into Mark in very early times? But here again we are faithful to the Lower Chriticism and insert the spices and ointment.

Another point. When Dr. Lake says: "ἐλϑουσαι εἰς το μνημειον need not mean more than 'when they came to the tomb,' " does he not understate the case? Can we not confidently say that they do not mean more? Thayer, in his lexicon, long since pointed out that εἰς το μνημειον in John xx, the parallel passage to the one in Mark, means simply "unto the tomb." In verse 1, the Revised Version renders the phrase: "unto the tomb"; in verse 3, "toward the tomb"; in verses 4 and 8 "to the tomb." So too in John xi. 31 and 38. In New Testament Greek, therefore, εἰς το μνημειον means "unto the tomb," and in order to introduce the idea of entrance, Luke and the copyists of Mark had to alter ἐλϑουσαι to εἰσελϑουσαι. Then, having gotten the women into the tomb, they must be gotten out again; hence the correlative corruption of ἀϰουσασαι ἐφυγον into ἐξελϑουσαι ἐφυγον, which in the Sinai Syriac is even more tautological:

they went out and went.

This text is a conflation, for it has already given the original reading known to Eusebius in his Cæsarean manuscripts:

And when they heard.

In fact, a close study of the documents reveals the fact that the whole passage has been systematically tampered with.

Mark xvi entire, as in the Oldest Manuscripts.
Revised Text.

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230 VIII And when the sabbath was past, Mary Magdalene and Mary the daughter of James, and Salome, brought spices, that they might come and anoint him.

231 I And very early in the morning, the first day of the week, they came unto the sepulcher at the rising of the sun. And they said among themselves: Who shall roll us away the sepulcher stone? (for it was exceeding great). And when they looked they saw that the stone was rolled away. And coming unto the sepulcher, they saw a young man sitting on their right, clothed in a white robe; and they were bewildered.

232 II And he saith unto them: Be not bewildered; ye seek Jesus the Nazarene, who was crucified. [He is risen; he is not here.] Behold, there is his place where they have laid him. But go your way, tell his dis-

ciples and Peter: I am going to Galilee ahead of you. There shall ye see me, as I have said unto you.

233 And when they heard, they fled, and said nothing
II to any one, for they were afraid of.............

HERE ENDETH THE GOSPEL OF MARK.

The numbered paragraphs are known as the Ammonian Sections and appear in the Sinaitic manuscript and down through the Middle Ages. Underneath the Hindu numeral is the canon ascribed to Eusebius, which is numbered in Roman. These canons represent an ancient Gospel analysis of no mean ability: Canon I means that the section is common to all four Gospels; II, to the three Synoptists; VIII, to Mark and Luke; X, peculiar to one; the rest to different pairs.

RECAPITULATION.

From early manuscripts, versions and Fathers we can reconstruct the text of the Resurrection in Mark, so as to read, at xvi. 5, "coming unto the sepulcher," instead of "entering into" it. Comparison of these authorities reveals the fact that the text has been systematically corrupted in early times by altering "coming unto" to "entering into," and "when they heard," in verse 8, to "going out of." This has been done so as to make it appear that the women found the grave empty, implying the doctrine of a fleshly resurrection. By the Lower Criticism alone we are enabled to correct these corruptions and to show that Mark originally contained no account of a physical resurrection. The familiar problem of the lost ending of Mark is incidentally dealt with.

ALBERT J. EDMUNDS.

PHILADELPHIA, PA.

BERGSONISM IN ENGLAND.

M. BERGSON has told us that on the arena of Europe to-day we have a spectacle of "life" in arms against "matter." He takes the two terms to express respectively the spirit of his own nation and that of the enemy. We may take them as expressing at least the alternatives between which his own philosophy moves. On the arena of the universe as a whole life and matter are in conflict and his philosophy seeks to decide between the two. Which is the ultimate reality? Which is to be explained by the other? Is life a product of matter or is the truth the other way, has matter itself been created by life? Bergson's works may be considered to constitute an elaborate answer to this question, and to decide it in favor of the latter alternative. He thus takes rank as a champion of the living against the dead. But the main position against which he argues—what one could call roughly scientific naturalism—is one with which philosophy in this country has long been accustomed to deal, and with a like result. Now that Bergson's philosophy may be said to have struck its first roots in this country, the time seems opportune for a comparison. We might with profit compare what he has to say with what the philosophy of the past two generations has been teaching. From some points of view it seems as though his special followers did not realize sufficiently the likeness of the two ways of thinking. Nor do they seem to have seen as yet how much some of Bergson's most valued positions seem to invite and indeed to demand strengthening by reference to matters which have been developed at least partially by the older idealism, and which are absent from him.

The opportunity for making the comparison is ready to our hand. As is well known, the head and front of Bergson's English following has come to be pretty much identified with Dr. H. Wildon Carr. While his work, *The Philosophy of Change*,[1] shows the influence of Bergson at every turn, and while Dr. Carr does not conceal his conviction that Bergson's is the most successful attempt yet made to deal with the questions of metaphysics, still it would be wrong to say that in this work he has merely been expounding another man's thought. He makes the endeavor to restate Bergson's central principle and to substantiate it by fresh applications; and a glance at the main topics will serve, we believe, to give a fairly true view of one important feature of Bergson's thinking which it is necessary to take account of, if we would bring its relation to English thought properly into focus. It will show us the *kind of questions* which figure in Bergson's pages.

The recurring topic is matter and spirit and the problems which arise out of their relation. For instance, is mind produced by the brain? If so, how could mind ever get at what is outside of this brain? for apparently mind can reach not only what is outside of it but what is even separated from it by immeasurable gulfs of space and time. Or, what are we to make of the question as to the constitution of matter, now that recent discoveries seem to resolve the material atom into something that is not a substance at all but only so much electricity? Then there are the problems connected with the relation of conscious mind to movement. Why is it that a higher form of consciousness, in the animal world, is so constantly accompanied by an increased capability on the animal's part of choosing its movements? There is further the nature of life itself. How are we to construe the fact that the whole past of every living being appears to be recorded in its present structure? And again there is the law of evolution and its ways of working. How is it that in a material world supposed to be governed by mechanical law, evolution can bend and govern the most dissimilar series of conditions so as to produce a like result—as when (to cull an illustration from Bergson himself) the series of conditions which produces a mollusk and the very different series which produces a vertebrate animal should both alike end in the one result, the endowment of the creature with an eye?

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[1] Macmillan & Co., 1914.

An outstanding characteristic of the whole way of thought will be apparent from this cursory survey. Its leading questions are such as would arise in the course of the study of natural science. They are questions which would occur to a scientist with philosophical interests. No doubt the motive which impels the mind to raise them lies far back in the perennial human sources of the philosophizing habit. But the actual questions raised come straight out of modern physics, mathematics, biology and psychology. They don't arise out of a study of the history of philosophical disputes. This is characteristic of Bergson. He is preeminently one of the writers who attack problems, not other people's solutions of problems. Hence his philosophical freshness. He does indeed deal with the history of thought. He deals also with some of the standing controversies of current philosophy. But these are not the center of his interest. He does not begin with these. His speculation thus acquires an interest for scientific minds which most philosophy does not possess. And it is perhaps not altogether fanciful to say that this feature gives his thought from the outset a certain advantage with the English mind. Moreover, in Dr. Carr Bergson seems to have minds also in another way, a way reminiscent of Schopenhauer as well as of the neo-Kantian idealism of Green. That consciousness is not preeminently representative or pictorial. It is active. Not the static picture of knowledge is its characteristic expression, but the energy of will.

In consciousness so conceived, then, Bergsonism finds the key to the broad facts of life and evolution as science has revealed them to us. In the evolutionary history of life on this planet—in the genesis and progress of vegetable world, animal kingdom and man—what we have is this active consciousness in the form of life, pushing itself, as it were, through the surface of matter and seeking free way. Man's physical organism is the one configuration of matter through which it finds the free course which it seeks. The human body is organized for giving outlet to this activity. The brain and nervous system are but its cutting edge by means of which it thrusts itself forward. The story of evolution is the story of how the main current of this vital impulse has worked its devious way through matter. The different forms of life which we see are the different channels into which the central stream has split itself up in process of thrusting itself into matter. The central stream has not quite dissipated itself as yet into these branches. The main current is still traceable. It is found in the life and consciousness of man. It is for this reason that man is at the head of creation. His life and mind contain the most complete concentration to be found anywhere of what was in the original world-impulse. The fundamental reality of the universe, then, is life; but it is a life which comes to view best, not in the plant or the animal but in the *conscious* life of man. We must note further that the "matter" through which the stream of life thrusts itself is in the last resort its own creation, though we need not go into Bergson's proof of that here. The vital impulse is thus creative of matter and of all the forms of life in which it finds an outlet, and the whole process of its advance is named by Bergson "creative evolution."

Some such view is the only one capable, in Bergson's opinion, of meeting the necessities of the case which natural science has presented to us. Dr. Carr has endeavored to go further and show that as a general view it is specially in harmony with some quite recent scientific discoveries. The "vital impulse" is nothing if it is not movement. It is, in fact, pure movement. If it be creative of "things" then somehow things must be generated out of movement. And this, Dr. Carr points out, is precisely what science is now finding.

"The essential principle of the philosophy of change," he says, "is that movement is original. Things are derived from movement, and movement is not a quality or character that things have added to themselves."[2] "A very few years ago," he says again, "such a doctrine would have sounded paradoxical and absurd. But now compare the philosophical doctrine of original movement with the new theories of science. Let us take first the structure of the atom. The electrical theory of matter teaches that the atom is composed of a central mass or core, which is far the larger part of its substance, and an envelope small in comparison. The central core is positive electricity, and the outer envelope consists of negatively electrified particles held in position by the electrical relation to the central core. The atom, in fact, is a solar system in which the positive element is the sun and the negative element the planets. And all the qualities of atoms depend upon the arrangement of these outer negative elements. But what is the ultimate reality of this atom—something or other that is electrified? No, it is electricity, not something electrified, and electricity is a form of energy, and energy degrades and disperses.

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[2] *Philosophy of Change*, p. 11.

Reduced to simple everyday concepts it is this, that what we call matter is a form of movement."[3]

But it is not merely in the case of the atom that recent discoveries have tended to resolve into terms of movement what we had been accustomed to regard under quite other terms; elsewhere also they have begun to transform the static into the changing, the resting into the active.

"But now turn to the other side," Dr. Carr continues (pp. 17-18). "In the last few years it has been possible to demonstrate that our solar system is not, as was supposed, at rest in an absolute space or else moving, if it be moving, without regard to forces outside itself. It belongs to a larger system, all the parts of which are in movement in relation to one another. The fifty million stars that our telescopes reveal are not scattered at random over the firmament, but are moving along regular courses coordinated to one another. The members of this stellar system are not, like the planets, revolving round a central mass, but millions of suns are streaming across an unoccupied center. The speed of our sun (now about 12½ miles a second) has been calculated, and its direction and the acceleration it will undergo as it travels across the center and passes outward again to the periphery. This, however, is not all. A discovery has been announced that seems likely to extend indefinitely further than astronomers have yet imagined the vastness of the spatial universe. Observations which have been made on the great spiral nebula in Andromeda show that its spectrum is inconsistent with the hitherto generally held supposition that it consists of gaseous matter in a state of extreme tenuity. It is now said to be a spectrum that is given out by solid glowing masses, and thus seems to confirm an old view that the nebulae are star groups immensely distant. This nebula is apparently not within our stellar system, but itself a vast stellar system lying outside the latter and at an enormous distance away from it. What other systems lie outside these we do not know, but all that we discover suggests universal movement. There is no absolute rest. If we conceive an observer placed anywhere in this great universe that we look out upon from our position on an insignificant planet of an insignificant sun, whether we suppose him to gather into one embrace what to us are vast stellar systems or to be confined to the negatively charged ion of an hydrogen atom, there will stretch out for him on either side an unlimited expanse of reality of which the ultimate essence is movement."

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[3] *Ibid.*, pp. 16-17.

And Dr. Carr finds a suggestion of the same point, viz., that things are not more original than movement but that movement is more original than things, in the way in which the recent "principle of relativity" in physics threatens to transform our conceptions of space and time and remove the ether from its place as a scientific hypothesis. All this seems to him to confirm the view expressed by Bergson in *La preception de changement*: "Movement is the reality itself, and what we call rest (*immobilité*) is a certain state of things identical with or analogous to that which is produced when two trains are moving with the same velocity in the same direction on parallel rails; each train appears then to be stationary to the travelers seated in the other." And again: "There are changes, but there are not things that change; change does not need a support. There are movements, but there are not necessarily constant objects which are moved; movement does not imply something that is movable."

The discovery that the whole universe is movement is, however, very little, if we know nothing about this movement except simply that it moves. Even when we have brought it so far that we can regard this movement as life, as creative, and as able at last to burst into consciousness, we have not even then got very far philosophically unless we learn something of the inner character of this vast spiritual force. On its inner character too must rest our judgment upon Dr. Carr's bold claim for the "new method," that it is nothing less than a revolution and that it has reversed the direction that philosophy has followed throughout its history of 2500 years.[4] It is on the subject of the inner character of this movement that Bergson's teaching most directly challenges comparison with that of Green; and, we may add, most clearly demands to be supplemented by it.

For Bergson is by no means the only teacher who has conceived of a universal spiritual energy as sustaining the universe. Green teaches the same. In the words of by far the ablest existing short exposition of it, the central conception of his philosophy "is that the universe is a single, eternal, activity or energy of which it is the essence to be self-conscious."[5] Nor can we get a distinction for Bergson out of his repeated claim that the spiritual activity of which he is thinking is not purely conceptual, because Green in essentials makes the same claim. A great deal is made by Bergson of the non-conceptual character of true philosophical apprehension. You cannot apprehend that ultimate essence or spiritual force whereby the universe exists, in the ordinary way; that is, by the intellect. The reason why, is that the intellect can only apprehend what is dead, static, given. It cannot grasp living movement. Now Green has quite as little use as Bergson has for what can only grasp the given or static. Natural science, for Bergson, is based on the intellect and that is why it cannot conduct us into the presence of what verily explains things.

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[4] See *Philosophy of Change*, p. 20.

[5] R. L. Nettleship in his biography of Green: Green's *Works*, Vol. III, p. lxxv.

Science only sees in the universe what is dead, and therefore it cannot exhibit its ultimate spiritual essence. This is Green's complaint too. Green, indeed, does not say that we must appeal to something else than the intellect (this is Bergson's way of putting the matter), but he does say that we must understand the intellect. We must not be content to use it uncritically, as natural science and naive common sense do. We must lay hold of life and activity with it. But Green is clear that the life and activity of which the intellect must lay hold, is its own. His philosophic creed, shortly stated, is that this is possible—that intellect, itself an energy, will reveal a spiritual energy at the heart of the universe, if it be persevered with and rightly used. Green does not say so in these words. But his philosophy says so to any one who has entered into it. He insists on the one hand that reason and will are one, in the sense that they are alike expressions of one principle;[6] and he speaks constantly of that principle as active and as self-active. His phrase is "self-realizing." On the other hand, his whole contention against the empirical school in philosophy was that this self-activity, the essence of men's minds, was not in men's minds alone. It was the essence of the universe. The spiritual principle was "in knowledge," and also it was "in nature," as the most elementary student of his chief work soon learns to know. Practically all he had to say about nature in fact was just this: that it was not inert, dead, merely given; that it was a spiritual life, of which our individual minds were the highest finite manifestation. So far Green and Bergson are on common grounds.

There is, however, a real divergence between Bergson's and the older teaching. They differ in their doctrine of time. Both agree that what we can see around us with the bodily eye is not the ultimate spiritual energy but its manifestations only. Even with the eye of the mind, they both hold, the ultimate spirit itself cannot be apprehended in its whole nature, for only part of its original totality is contained in the mind of man. But they differ as to what it is in its own whole nature. With Green it is in itself already perfect, whereas with Bergson it is still developing and changing with the course of time and has an immense and entirely uncertain future ahead of it. With Green the ultimate spirit is complete, with Bergson it is incomplete. With Green it is a consciousness, morally and intellectually all that we could conceive ourselves becoming. With Bergson it is a consciousness still always turning into something different and turning always into something which could not have been predicted.

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[6] See, *inter alia, Works,* II, p. 329.

At this point also occurs the most marked difference of the two doctrines in regard to the light which they cast upon the assumptions of the moral and religious consciousness. And as regards religion, it is not hard to judge which is in the stronger position. So far as the religious mind has entertained the belief that behind the phenomena of the universe and acting as their source, there exists a mind which is eternal, one who is above time and vicissitude, who is perfect and is not subjected to change, a God "who was and is and ever will be," in so far it will find its faith countenanced in Green's teaching but discountenanced in Bergson's. Dr. Carr himself is clear that the change Bergson's theory invites us to make in our religious conceptions is profound, though he thinks that it has compensations.

"How is the conception of God affected by the principle of this new philosophy? One attribute that has seemed to attach to this conception can certainly not belong to it—eternity, in the sense of timelessness. Reality is essentially movement, movement is duration, duration is change. If we call the original impulse of life God, then God is not a unity that merely resumes in itself the multiplicity of time existence, a unity that sums up the given. God has nothing of the already made. He is not perfect in the sense that He is eternally complete, that He endures without changing. He is unceasing life, action, freedom.

"No more profound change can be imagined in the conception of the universe, in the conception of human nature, in the whole outlook of life, than is involved in this new conception of God. The conception of God to which we have been accustomed in philosophy,—the most perfect being, the *ens realissimum,* the first cause, the *causa sui,* the end or final cause,—is the conception of a reality which time does not affect. Hence the continual attempt both in ancient and modern philosophy to conceive two orders or kinds of existence, the temporal and the eternal, and the whole problem of philosophy has been to conceive the relation of these two orders to one another. Time and the whole order of changing reality must, it has seemed, be of the nature of an emanation from God, or a manifestation of God. But however conceived, the time order is regarded as essentially unreal, appearance and not reality; change and movement are relative to us."[7]

Connected with the same difference in regard to time, there is again a difference of the two theories as regards the light they throw upon another of the Kantian postulates. So far as the religious consciousness has fixed its hope on immortality in the sense of a life out of time Bergsonism can offer no corroboration, for time and the change which constitutes it are to this philosophy reality itself, and to be out of time is *ipso facto* to be out of existence. Here again the older view is considerably different. For it the idea of a life beyond time is at least not contradictory. Nay, any completing or perfecting of our best life here would inevitably have this character, since for this view we are already above time in so far as we think what is true and do what is unselfish.

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[7] *Philosophy of Change,* pp. 187-188.

As regards the postulates of God and immortality, then, the effect of Bergsonism is of a negative character. But these are preeminently religious postulates. The point upon which Bergsonism claims most confidently to have substantiated our higher emotional demands is in regard to the moral postulate, that of freedom. In its clearness upon this question, indeed, Dr. Carr finds the chief compensation for its attitude upon the others.

"The philosophy of change does not sound any clear and confident note as to what lies beyond us in the unseen world. It does not present to us God as the loving father of the human race, whom He has begotten or created that intelligent beings may recognize Him and find happiness in communion with Him. There may be truth in this ideal, but it is no part of philosophy. Neither does it teach us the brotherhood of the human race—on the contrary it seems to insist that strife and conflict are the essential conditions of activity. Life is a struggle, and the opposing elements are the nature of life itself, the very principle of it. The evolution of life is the making explicit of what lies implicit in the original impulse. Philosophy reveals no ground for the belief in personal survival, and it shows us that however highly we prize our individuality we are the realization of the life-impulse which in producing us has produced also myriad other forms. What then is the attraction that this philosophy exercises? What is there of supreme value that it assures to us? The answer is freedom."[8]

Here at length we reach the philosophically important matter. For here we can interrogate the two views, not merely as to whether they can corroborate our religious

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[8] *Philosophy of Change,* pp. 195-186.

sense, but as to their grounds for doing so.
question for the critical evaluation of the ph
Bergson, it may be said, is that of the nature
evidence for the freedom which he says charact
ultimate spiritual force of which we are the offs
which by its vast uprush through the universe and through us creates us and the universe as it goes.

For Green too, as everybody knows, there is freedom. And he puts the *rationale* of it thus. Man is free, for him, both in his knowing and his acting, because in both of these functions the past is gathered up in the present which is now before him. Except this were so, says Green, we could not know. To know, is to know succession. Now if there were only succession itself—that is, if the past were not thus gathered up—there could be no consciousness thereof. This is straightforward reasoning, and at bottom quite simple. If I am gathering a bunch of flowers, I must hold the first ones in my hand while I gather the rest. If I did not do this but dropped each one as I picked it, I should never have a bunch. Quite similarly, if I hear or see a succession, say, of strokes upon a knocker, and if I *know* that it is a succession of five knocks, my knowing is evidence sufficient that the earlier strokes have not escaped me but have been gathered up in my mind and presented along with the last one. If each had disappeared as it occurred there would have been no succession of five for me. Each one would have been number one; and when it was over would have been nothing. To perceive time at all I must not merely have the present before me. I must have the past along with the present. In Green's phrase, the various members of the series must be "co-present" to consciousness.

Bergson has made an analysis of this same experience, and has given the matter profound attention. He too sees, that to know succession in the ordinary sense of knowledge the members of the succession must be somehow co-present, but he gives the whole matter another turn. He cannot feel, apparently, that in knowing the successive as thus co-present we are really knowing the successive at all. His refrain therefore is, we *try* to know a time-succession by the ordinary use of our intellect, but cannot. We do not, in this fashion, know a time-succession. We only succeed in knowing space. In counting the strokes we set out the series of events in a row, along a line, in a kind of mental space. This we call perceiving their temporal succession. And if one asks, "Why does the intellect fail? how are we to apprehend time, or what would it be like if we would apprehend it?" the whole argument of Bergson's *Time and Free Will* converges in effect upon this answer: that the intellect which fails to apprehend time-succession fails because it can only set out the events *separately* along an imaginary spatial line, whereas for the "intuition" which really apprehends time these events are not separate, they interpenetrate. This interpenetration *is* time. It is fairly easy to see further how, out of the apprehension of such time, he gets free will. We have to pull ourselves together in order to grasp this interpenetration; and in this attitude, in this tense summoning of ourselves together, we are free.

We have here the fundamental impeachment of reason to which Bergson's philosophy seems compelled to have recourse. To reject the intellect as a means of attaining to the truth is an obvious weakness, as compared with the other view, thus far—that it is a species of self-subversion which the view with which we are contrasting it does not commit. Both Bergson and Green in philosophizing at all are endeavoring to settle their account with the problems of life by thinking them out. Both, in other words, are making use of the intellect. The difference between them in regard to the matter before us is that Green trusts the instrument he is using. Having found what the intellect perceives time and succession to be, he says frankly that that is what they are. But Bergson, unable to accept the verdict, will rather make bold to say that our rational mind is incompetent, that it is incapable of seeing things as they are, and so has no authority in the case. This is a serious matter. One cannot feel, after this, that the intellect can be a very safe instrument to philosophize with. This is perhaps the rock on which all philosophies eventually split which attempt to reason the reader into preferring some supra-rational or sub-rational power before reason itself. Mere reason may not be fit to see what reality is; but if not, is it fit to attack itself either? We cannot endorse this intellectual abuse of the intellect. If the intellect cannot justify itself it cannot justify anything. We must accept the intellect, or our whole attitude is sceptical.

"But the intellect can't allow you free-will," it will be at once objected. This is an ancient objection, of which, as we shall see, Bergson himself shows us how to get the better. What, we have to ask,—what precisely is the freedom that Bergson's argument itself will bring us if it is true? It is easy for Dr. Carr to speak as if Bergson preserved for us the privilege of a wide choice in an open universe. All defenders of freedom have used such language. The question is, what evidence has he? What is there in our own experience that we can fall back upon and see that the universe is open before us? What reveals our identity with a universal principle of freedom which creates the universe itself, and in whose life we are free?

Whatever answer can be got out of Bergson to the question must come from the "interpenetration" just mentioned. And on inquiry we find that it is a solid answer enough. We do get evidence of freedom. And it is from the "interpenetration" that we get it. Bergson is one of the few people who see where the freedom issue really lies.

In *Time and Free Will* he insists that freedom is to be looked for in the character of an act itself. It is the question "what was the act?" that is essential; not the question "what might it have been?" or "could it have been different?" What we have to ask about two alternative courses of conduct ahead of us, when we want to know whether we are free agents, is not "is either equally possible to me now?" but "what is the inner character of the one chosen when it does eventuate?" And he indicates, in language which might have been copied from Green, that our character must be in our act. "We are free," he says in *Time and Free Will*,[9] "when our acts spring from our whole personality, when they express it, when they have that indefinable resemblance to it which one sometimes finds between the artist and his work. It is no use asserting that we are then yielding to the all-powerful influence of our character. Our character is still ourselves;" etc., etc. And what we learn from his lengthy subsequent discussion of the matter is simply this: that where "interpenetration" occurs, there our character is; where the multiplicity consciously present in us is made up of items which interpenetrate, there our personality has its seat. And where the multiplicity of interpenetrating states is at its maximum in the great, critical decisions of our life, there our freedom is at its maximum because our personality is so. "It is the whole soul. . . . which gives rise to the free decision; and the act will be so much the freer, the more the dynamic series with which it is connected tends to be the fundamental self."[10]

It takes a great effort, often, to draw the scattered multiplicity of our conscious states into this interpenetrating unity. And in his later work, *Creative Evolution*, Bergson tries to show that when this concentration of spirit is relaxed an order of freedom transforms itself into an external order of necessity. There is no disorder in spirit, but only these two opposite kinds of order. That is how he accounts for matter. It is the de-tension of the universal life-impulse. But the present point is, that an act is free when our personality is in it, and that happens when it is one such as gives outlet or utterance to a multiplicity of states held in an intense interpenetrating unity.

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[9] English Translation, p. 172.

[10] *Time and Free Will*, p. 167.

So far, Bergson conducts us along safe and solid ground. But let us not make a mystery of this interpenetration. The highest examples of it are to be found only rarely, no doubt. We find them in moments when the entire being of a richly endowed mind, all its desires, fears, hopes, knowledge, emotions, converge in one direction, meditate one high and hard decision, and that decision is taken. There you have that contracting together of the entire soul for the effort, of which Bergson speaks under so many similes, and which is perhaps the highest act of a life. But there are simpler examples. The simplest is the common experience we have already referred to—the mere watching a series of events go by. The vague impression left by the last "click" of a series to which we have not been attending will tell us, says Bergson, (if we start up afterwards and try to count how many we have missed) when we have counted enough. In such a case the objects we consciously count are set out in a sort of mental row. Not so the vague impression which acts as our standard and says to us when we have counted up, say, four, "that is enough." This vague impression does itself contain four. It is an impression of four. But it contains them in a different way. In it they are not set out in a row, but interpenetrate. Its "four" character, its quadruplicity if you will, is a unique quality.

"Whilst I am writing these lines, the hour begins to strike upon a neighboring clock, but my inattentive ear does not perceive it until several strokes have made themselves heard. Hence I have not counted them. Yet I only have to turn my attention backwards to count up the four strokes which have already sounded and add them to those which I hear. If, then, I question myself carefully on what has just taken place, I perceive that the first four sounds had struck my ear and even affected my consciousness, but that the sensations produced by each one of them, instead of being set side by side, had melted into one another in such a way as to give the whole a peculiar quality, to make a kind of musical phrase out of it. In order, then, to estimate retrospectively the number of strokes sounded, I tried to reconstruct this phrase in thought: my imagination made one stroke, then two, then three, and so long as it did not reach the exact number four, my feeling, when consulted, answered that the total effect was qualitatively different. It had thus ascertained in its own way the succession of four strokes, but quite otherwise than by a process of addition, and without bringing in the image of a juxtaposition of distinct terms. In a word, the number of strokes was perceived as a quality and not as a quantity; it is thus that duration is presented to immediate consciousness, and it retains this form so long as it does not give place to a symbolical representation derived from extensity."[11]

Now the freedom which Bergson secures, and which he says cannot be apprehended by the intellect but only by what he calls "intuition," is this interpenetration. The intellect, he holds, cannot grasp it. But if we put aside his statement that the intellect cannot grasp this unity of interpenetrating items, and attend solely to his description of what the intellect is alleged not to be able to grasp, we find that his statement is quite wrong. The intellect can grasp it, and Green's doctrine is precisely that it can. True, "interpenetration" is not a favorite word of Green's. He speaks of relation. He holds that the members of a succession in order to be known to our minds as a succession must be related; so related that they are co-present. But this interrelation which Bergson says is a misreading of time and a translation of it into mere "space symbolism" because the members don't interpenetrate, this intellectual apprehension of a succession, is already to Green precisely a complex of interpenetrating elements. True, the items are connected by relation, but relations are internal for Green. They are constitutive of the thing's character. The relations in which each thing stands to the others are what make its nature. The nature of all the others, therefore, enters into each, and that of each into all the others. They must interpenetrate; their natures do so as truly and literally as two brushes which have been stuck together. The fact is, it is altogether the same whether we say of certain elements that their mutual relations are internal to each of them, or that they penetrate one another.

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[11] *Time and Free Will,* Eng. Trans., pp. 127-128.

"But this is not the interpretation that Bergson means," it will be replied at once. "This interrelation of Green's would never yield anything like freedom. What Bergson means is a vital interpenetration, not any dead static thing such as could be illustrated by the mere material interpenetration of the bristles of two brushes." Entirely so. The metaphor does not do justice to Bergson's position, and neither does it to Green's. With Bergson the interpenetration of the elements seen by "intuition" is vital, it is an intense living movement, and he strains language to express how the elements fuse together, melt into each other, inter-work and support a real life. But neither, with Green, are the objects of *the intellect* in a dead *relation.* A relation, with him, is a relating—a living activity, therefore. He has nothing to teach if he does not teach this. He has nothing to urge if he does not urge that a system of relations "implies a relating mind." And surely no one ever took him to mean by that, that the implied "mind" merely made the system, set it down, and left it for ever alone to stand there, dead, cold and finished. The relations are alive. They are being kept up. They are a deed; and not a deed done but a deed ever a-doing. A relation of two things, with Green, is a supporting of them in an energy of ceaseless spiritual movement, in precisely the Bergsonian sense.

"But this movement constitutes the things, with Bergson; it is their source, the very stuff of which they are made." Even so with Green, and much he has been made to suffer for it! It is not, says Bergson, things which are first and which come to interpenetrate afterward. It is the movement or interpenetration which constitutes the things. It is not, says Green, things which are first and which come to be related or interpenetrated afterward. It is their relation or interpenetration which constitutes them. A thing is nothing apart from its relations.

So far as regards the tracing of reality to a spiritual source Bergson indeed uses a language which is different from that of the older idealists. But in this general matter his fundamental thought is accurately the same. The only difference is that the older teaching does not fall back on any special intuition in order to be assured that reality has a spiritual source. It relies on the more thorough application and the critical use of the intellect itself. It holds that this most important of truths still is truth, and that by those who persevere it may be reached by the same methods through which other convincing truth is reached, namely, by the exercise of reason.

But this one difference is a difference as of heaven and earth. By disparaging intellect it puts Bergson in the unhappy position of constantly needing to discredit that very faculty of "reasoning" upon which as a philosopher he must stake his own results; and that is not the whole of the trouble. It also gives a false cast on the moral side to the entire physiognomy of his teaching. And with a glance at this we may close our review.

The significant point is that Bergson does not believe in the intellect, or in the typical object of the intellect, namely, space. By not believing in them we mean that he does not believe in their spirituality. Green does. Green finds in space itself that very interpenetration or spiritual movement which Bergson insists cannot be found there. He finds, that is to say, in the (spatial) object of the intellect something which fully answers the essentials of Bergson's description of the interpenetrating, while Bergson constantly speaks of this character in things as though it could not be seen at all intellectually, but only in glimpses, by the special power of apprehension which he calls intuition. Green, in a word, finds in the spatial-intellectual that reality and truth which Bergson can only find when all "space-symbolism" has been done away with. This is a serious difference. For this "space-symbolism," in the wide meaning which Bergson gives to it, is the very stuff and fiber of the moral life. His teaching therefore means that to be at the moral point of view is to be out of touch with the real truth of the world.

And unfortunately his actual ethical teaching bears out the suggestion. It is quite a mistake, we may note in passing, to say that Bergson has not written on ethics. It is true he has not written any book with that name. But he has a work the real burden of which is an interpretation of the moral and social life. This is his little treatise *On Laughter*. His thesis in that work is that laughter is a species of social castigation. It is designed to rid society of the conduct that provokes it. And the question for the moral implications of Bergson's teaching is, what is it whose destiny is thus to be socially castigated? Startling as the answer may seem, it is the moral. It is called the mechanical. In the wide sense in which Bergson eventually uses the term, it is the intellectual-spatial. But in the concrete what is it? It is simply faithfulness to principle where such faithfulness is awkward. In other words it is the very soul of the moral life, if that is anything at all distinct from the "esthetic" life. This disbelief in space and the spatial, this disbelief in the negation which is at the root of these, is what the present writer has ventured to call the pessimism of Bergson.[12]

Without repeating here what has been worked out elsewhere,[13] reference may be permitted to one little point in elucidation of this view. It concerns Bergson's first illustration in *Laughter,* his picture of the runner who stumbles and falls. It is a small matter, of course, but it has always struck the present writer as a peculiarly significant accident that Bergson should have opened an essay *On Laughter* by taking as his first example of the ridiculous precisely that figure which has served so many moralists for their type of the moral life. The runner of Bergson's illustration, as Bergson describes him, with his eagerness and his "rigidness," with his omitting to look where he is going, his stumbling over obstacles and his abundant inability to adapt his conduct as circumstances require, and follow the sinuosities of his crooked path, is indeed ridiculous. But it is only Bergson's light vein that makes him so. There is nothing essentially ludicrous about such a man. In essentials, he might be Bunyan's pilgrim fleeing toward the wicket-gate or St. Paul's runner, who also heeds nothing either right or left, but simply "presses toward the mark." Of course there would be nothing in a mere illustration as such, but this one is so absolutely well chosen. This *is* the type of man—this steadfast man, this man who just is *not* sinuous and yielding and pliable and graceful and free, this straight-going individual who cannot do anything but go straight—*this* is the type whose proper destiny, according to the whole tenor of the essay, is to be laughed out of society; this is the man for whom society has no use. "Since when?" some may feel inclined to ask, not without a tinge of indignation. We confess that to us, hitherto, society has seemed to have considerable need for him; nay, to have had, perhaps, prodigiously little use for the other sort in comparison.

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[12] See articles in *The Hibbert Journal* for October 1912, *The International Journal of Ethics* for January 1914 and *Mind* for July 1913. Compare an article on "Bergson, Pragmatism and Schopenhauer" by Günther Jacobi in *The Monist,* Vol. XXII, pp. 593ff. The latter article, however, should be read with caution. The present writer has the best of reasons to believe that the marvelous correspondence in detail which exists between Bergson and the prince of pessimists is largely accidental. Bergson himself learned about it only after his own principles had been evolved into practically their mature shape.

[13] In the article in *The International Journal of Ethics* referred to.

Moreover it is the discovery of precisely what this social theory neglects, namely the spirituality in spatiality itself, that enables the idealist to endorse the religious consciousness of God as eternal and perfect, without losing the other point, equally important, that the divine nature must also be movement, activity, freedom. To science the natural-spatial world is a completed order. If such order implies spirit, then, there must be a completed mind. As for the compatibility of such completeness with freedom, the very reasons which make Bergson to see real, active, free spiritual life except in a present which has the past in it, make it impossible for the idealist to see the perfection of such freedom except in a living present charged not only with the whole past, but with the whole future as well. The whole of reality must interpenetrate as Bergson makes the reality which has so far elapsed do. That interpenetration, with its inner activity, movement and freedom, makes up the content of what the religious consciousness has conceived as the perfect mind of God. Its inward intensity is God's perfect life, which is also ours so far as we are both good and great.

With the claim then, which is put forward by most of Bergson's following here and elsewhere that his philosophy is both true and "new," we cannot agree. So far as we have been able to examine it, it differs from other idealism in an essentially philosophical way only when it has something to say which is indefensible. Bergson has done important work in matters which in this paper we have had to pass over because they are extra-philosophical. He has done great work in psychology; and he has also done great work in the interpretation of the actual story of evolution, by bringing out new facts there which could easily be shown to be as compatible with the classical idealistic defense of spirit as with his own. That kind of work is the limit, it seems to us, of his service; except indeed it be a service to have presented a great deal of the substance of idealism from an angle so entirely fresh as almost to transport the reader into the idealistic center of vision, without his suspecting that he is there. We are not convinced that this is a small service. Nay, rightly understood, there is perhaps no greater.

J. W. Scott.

University of Glasgow.

THE PRESENT STATUS OF THE UNCONSCIOUS.

THE unconscious is a topic with which some writers have tried to coquet freely, which others have shunned scrupulously, and which still others have approached in a true scientific spirit in the endeavor to find out precisely what it is and how well it can explain the phenomena that usually come under its name. The motives that have led men to write about the unconscious have differed so widely that it is not surprising to find the works of some fantastical, and those of others useful for practical purposes only but devoid of scientific information. The interests of the former type have been purely metaphysical. Their object being to discover unity and continuity in the universe, they have postulated the unconscious as the absolute principle. The interests of the latter, who are chiefly physicians, have been entirely practical. Naturally they have considered and still continue to consider the subconscious from the functional point of view. What its real nature is and how it is related to consciousness as such, is a problem that does not fall within their sphere. The third group comprises the few psychologists who, motivated by a true scientific and progressive spirit, are seeking to discover the "what" and the "how" of subconscious activities. As a result of these diverse attitudes we find that one writer thought the unconscious a topic sufficiently great and all-embracing to deserve three large volumes, while another laconically dismisses it in three monosyllabic words. But the fact that we still have the problem on our hands shows that neither the three volumes of von Hartmann nor the three words of Münsterberg[1] have either brought us any nearer to its solution or diminished in the least our unavoidable duty as scientific psychologists to search out the cause of unconscious activities and, if possible, to bridge the gap between the conscious and the unconscious.

As an example of the psychologist who avoids all discussion of the subconscious, I need only mention Titchener, who, after defining the subconscious as "an extension of the conscious beyond the limits of observation,"[2] goes on to say that it is always a matter of inference and therefore "it can not be a part of the subject matter of psychology." It is merely employed as an explanatory concept, he declares, but there are two reasons against its use in psychology: First, that the scientific psychologist, like the scientist in general, is not called upon to explain anything; and secondly, the introduction of this inferential concept may lead to danger inasmuch as it is "impossible to draw the line between legitimate and illegitimate inference."

As an example of the thinker who interprets all mental phenomena in terms of the subconscious, I may mention von Hartmann and Schopenhauer. The former endows the entire universe with an unconscious mind, declaring it to be the absolute principle which operates in all things organic and inorganic. But as James says, "his logic is so lax and his failure to consider the most obvious alternative so complete, that it would....be a waste of time to look at his arguments in detail." Nor are the views of Schopenhauer much more reasonable. According to him every sense organ unconsciously infers its impinging stimulus "as the only possible cause of some sensation which it unconsciously feels."[3]

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[1] *Psychotherapy*, p. 125.

[2] *A Beginner's Psychology*, p. 327.

But the theory of the subconscious dates back farther than von Hartmann and Schopenhauer. Although Weingärtner traces it to Plato and Plotinus, we may say that it received its first definite formulation at the hands of Leibniz in his conception of the *petites perceptions* which play the main role in psychic activity. These subliminal perceptions are individually too faint to arouse consciousness, according to Leibniz, but in their totality they come to a high degree of consciousness. To use his own words, "the belief that there are no other perceptions in the soul than those of which it is conscious, is a great source of error."

Kant's view of the subconscious is somewhat analogous to that of some modern authors, particularly Lipps. He declares: "To have sensations and not to be conscious of them is a contradiction, for how can we know that we have them, when we are not conscious of them? But we may infer we have had a sensation or a perception, although we were not immediately aware of it."[4] Such perceptions Kant calls "vague," and their field he declares is much broader than that of the clear and definite ones.

Turning to the English school, we find Sir William Hamilton asserting that although he does not wish to maintain that all consciousness is the product of unconscious perceptions, and that knowledge as such is the product of the unknown and the unknowable, still we must confess, he says, "that there are things which we neither know nor can know directly, but which manifest their existence indirectly through the medium of their effects."[5] Hence, since the mind in its behavior manifests processes of which it is unconscious, these processes must have come about through some modification of mind, and this may be called the unconscious. Or as Carpenter believes, Hamilton meant by this "unconscious cerebration."

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[3] James, *Psychology*, I, p. 170.

[4] Soewenfeld, *Bewusstsein und psychisches Geschehen*, p. 2.

[5] Carpenter, *Principles of Physiology*, p. 518.

Maudsley undertakes to interpret the greater part of conscious behavior in terms of unconscious psychophysical processes. He observes that almost from the moment of birth the sensorium receives multifarious impressions which it assimilates unconsciously, and makes use of them in a purely mechanical manner even in so-called intelligent activity. Even our general and abstract concepts are developed unconsciously; in short, "the process upon which our thinking depends," he says, "goes on of its own accord, without our awareness."

Carpenter devotes a whole chapter to unconscious cerebration and declares that since there is reason to believe that the greater part "of our intellectual activity"—whether it be reasoning or imagination—is essentially automatic, it is not unlikely that "the cerebrum may act upon impressions transmitted to it, and may elaborate intellectual results, such as might have been attained by the intentional direction of our minds to the subject, without any consciousness on our own part" (p. 515).

This view was subsequently taken up by Huxley and made to explain all intellectual activity. Noticing that epileptics can execute complex actions without having any memory of them upon recovery, and also that somnambulists can write letters and compose original verse while in their so-called sleeping state, he concluded that since "these cases are examples of purposive and intelligently controlled action taking place without consciousness, it would seem to follow that the mere mechanism of the nervous system" is all that is needed for the execution of such actions, independently of all consciousness and conscious guidance; and, therefore, we are compelled to assume that when similar actions are accompanied by consciousness, the nervous mechanisms are the only essential conditions, and "consciousness is a superfluous accompaniment, so far as the causal sequence is concerned."[6]

It is obvious from the above that not only do earlier writers disagree on how the subconscious functions but they even differ with respect to its essential nature.

Turning to modern authors we find that among them, too, there are almost as many views as writers on the subject. On the one hand, men like Freud declare that subconscious phenomena are due to dissociated and suppressed ideas; that these unconscious ideas are active, though the individual may not be aware of them while going through the bodily actions of which those ideas are the prime causes.[7] On the other hand, Sidis retorts that the existence of unconscious ideas is inconceivable, for "ideas are essentially of a conscious nature";[8] hence their introduction into psychology is a self-contradictory concept. The subconscious, according to him, is rather "a diffused consciousness below the margin of personal consciousness."[9] Again, writers like Prince and James conceive of the subconscious as the outerlying fringe of consciousness, as dim consciousness, or better still, as the base of a cone, the apex of which is attentive consciousness. While Irving King refutes this view, declaring that consciousness either exists or does not exist, that "it may be more intense at one moment than at another.... But at any one moment it is.... a unitary existence without parts which might be thought of as clustering about a center with different degrees of intensity and adhesion."[10] Finally, while Sidis mocks unconscious cerebration, characterizing nerve currents, nerve-paths and neurograms as "figments of imagination,"[11] Ribot maintains that the psychological aspects of the subconscious play but a secondary role, that they are a result, an effect of physiological or neural processes.[12]

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[6] McDougal, *Body and Mind,* pp. 109-110.

[7] Freud, "A Note on the Unconscious, "*Proc. Soc. Psy. Res.,* XXVI, 1912, p. 314.

[8] Sidis, "The Theory of the Unconscious," *Proc. Soc. Psy. Res.,* XXVI, 1912, p. 337.

[9] *Ibid.,* p. 319.

[10] "The Problem of the Subconscious," *Psychol. Rev.,* XIII, 1906, p. 43.

No small part of the above controversy and disagreement is due to the fact that the term subconscious has been used in widely different senses. Prince gives no less than six different meanings in which the term has been employed.

1. The word subconscious has been employed to describe that portion of our field of consciousness which at any moment is outside the focus of attention. In this sense it is equivalent to James's fringe of consciousness.

2. The second meaning asserts that the subconscious is composed of ideas that are dissociated or split off from the personal consciousness, i. e., the focus of attention; that though the subject is unaware of their existence they are none the less active, and that "they form a consciousness coexisting with the primary consciousness, and thereby a doubling of conscious results."

3. According to the third meaning of the term, "subconscious states are conceived of as becoming synthesized among themselves, forming a larger self-conscious personality, to which the term self is given." These subconscious states are personified by the people who hold this view, and referred to as the "subconscious self" or "the hidden self."

4. The fourth view conceives the subconscious as "including all those past conscious states which are either forgotten and cannot be recalled, or which may be recalled as memories," their non-existence being due to the fact that they are crowded out of consciousness by the bulk of present experience.

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[11] *Op. cit.*, p. 325.

[12] "A Symposium on the Subconscious," *Journ. Abnorm. Psychol.*, II, 1907, p. 37.

5. The fifth view, which is that of Frederick Myers, "declares that subconscious ideas, instead of being mental states dissociated from the main personality, are the main reservoir of consciousness, and the personal consciousness is a subordinate stream flowing out of this great storage." .....In short, we have within us a great tank of consciousness, but are aware of only a small portion of it.

6. The sixth and final view asserts that there are no psychical elements in subconscious phenomena at all, that automatic writing and speech, the solution of mathematical problems in sleep, and the carrying out of post-hypnotic suggestion "are the result of pure neural processes," unaccompanied by any mentation whatever.[13]

Before presenting the detailed arguments in support of the above theories, let us hastily review some of the more common subconscious phenomena in order that we may have freshly before our minds the facts which these theories endeavor to explain.

The main test that a by-gone experience was accompanied by consciousness is memory.[14] The ability to recall an experience without the artificial aid of suggestion or abstraction, shows that the individual was conscious of that experience at the time he underwent it. But memory is composed of three factors: registration, conservation and reproduction. Something must be impressed on the sensorium in order to be recalled, and it must also be conserved in some form. The question therefore arises: Does every impression, however faint it may be, stir up a pulse of consciousness which is immediately forgotten because of its brevity or faintness, or can reproducible impressions be made without the least awareness at the time being? And if so, how are they conserved? Daily observation and laboratory experiments demonstrate that perceptions of the environment of which the individual did not have the least awareness, may be conserved. You may pass an acquaintance on the street without being aware of him at the time, but two or three minutes later it will suddenly dawn on you that you had seen your friend so and so. Again in hypnosis, by means of automatic writing or abstraction, people have been able to recall paragraphs in the newspapers read through casual glances without awareness thereof. Or the experiment may be put under controlled conditions, by having the subject take a brief survey of the room, and then while blindfolded dictate as detailed a description of it as he can. Thereafter if he is hypnotized and asked to describe the room once more, "it is often quite surprising," says Morton Prince, "to note with what detail the objects which almost entirely escaped conscious observation are subconsciously perceived and remembered."[15] Another method of proving the conservation of unconscious experiences is to have a person concentrate his attention by giving him something to read or an arithmetical problem to perform, and while he is so engaged to place cautiously and surreptitiously objects within his peripheral field of vision. After their removal he is asked to state in detail what he has seen. Invariably he is unable to mention any of these surreptitiously introduced objects. On being hypnotized, however, he mentions them with considerable accuracy and readiness.[16]

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[13] "A Symposium on the Subconscious," *Jour. Abnorm. Psychol.*, p. 22.

[14] McDougal, *Op. cit.*, p. 109.

Automatic writing furnishes another group of facts which presuppose subconscious processes. If into the anesthetic hand of an hysterical person a pencil be put the hand will commence to write mechanically, and the subject will observe the movements of the hand as if that member belonged to some other person. Nor will the patient recognize the written ideas as his, but if he is hypnotized he will claim them immediately and explain what he meant by them. Sometimes the two hands of the same subject may be made to give written expression to two different kinds of mental content.

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[15] Prince, *The Unconscious*, p. 53.

[16] *Ibid.*

Perhaps the most interesting and common source of subconscious phenomena is somnambulism. People in this state have been known to perform the most delicate feats of physical skill, such as walking across roofs on narrow planks. Others have been known to perform events that in waking life require a great deal of intelligence,—such as writing letters or verse. Yet they have no memory for these events. The question arises: Are these highly complex mental activities performed mechanically, without any mentation, or are they consciously performed, but forgotten in waking life because dissociated from the personal consciousness?

Post-hypnotic suggestion is no less a mystery than complete change of personality. An individual is hypnotized and is told that at a fixed time after he awakens—be it several minutes, an hour or a day later—he is to do a certain deed. He is awakened and asked if he remembers anything that had been said to him during the hypnosis. He does not. He is permitted to depart and goes about his business in his customary manner. But precisely at the fixed time he will carry out the post-hypnotic suggestion, whether it be to ask for a pail of coal in a jewelry store or to purchase an overcoat in summer. When he is asked why he did this he can only reply that something within prompted him to it, that he felt it was a voluntary deed.

Whether such a case as that of the Rev. Ansel Bourne would fall into the group of epileptic phenomena or not matters little. In both instances we know that the subject will go through many complicated activities, denoting a high degree of consciousness or the presence of the customary kind of intelligence as judged by the adaptation of the subject to his environment, yet in neither case does the normal personality have a memory for these experiences.

How are these phenomena to be explained in the light of modern psychology?

Two general theories are proposed: the psychical and the physiological. And it is to these two that the six foregoing views can be reduced after we eliminate Myers's metaphysical notion which conceives of the subconscious as the reservoir of all consciousness, and that other view which interprets the subconscious as the larger self-conscious personality.

Freud, Sidis and Janet may be taken as the chief exponents of the psychological theory of the subconscious, while Pierce, Jastrow and Ribot, not to mention a host of others, hold to the physiological view. The former trio in one form or another declare that the subconscious is dissociated consciousness, or awareness that is dissociated from the synthesizing personality, and that this awareness exists in consciousness in a latent form all the time. The latter maintain that not only is it unscientific to speak of latent ideas and latent feelings, but that there is no causal relation among psychic elements at all, that the explanation of unconscious phenomena must be sought in neural processes.

Let us examine their views individually.

Freud suggests that the term "conscious" should be applied to the perception which is present to our consciousness and of which we are aware,—while the latent perceptions should be denoted by the term "unconscious." "Hence an unconscious idea is one of which we are not aware, but the existence of which we are nevertheless ready to admit because of other proofs or signs."[17] This unconscious idea, though latent in the sense that it does not attain awareness, is by no means inactive while in the mind. That unconscious ideas are active, undergoing combination and recombination among themselves, is demonstrated by the hysterical patient. "If she is executing the jerks and movements constituting her fit," says Freud, "she does not consciously represent to herself the intended actions, she may perceive those actions with the detached feelings of an onlooker. Nevertheless, analysis will show that she is acting her part in the characteristic reproduction of some incident in her life, the memory of which was unconsciously present during the attack."

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[17] *Op. cit.*, p. 313ff.

Freud distinguishes two kinds of latent ideas: those which enter consciousness with no difficulty whatever, and those which do not penetrate into consciousness however strong they may be. The first type constitute the foreconscious, the second type the unconscious. "The term unconscious," he says, "now designates not only latent ideas in general, but especially ideas with a certain dynamic character, ideas keeping apart from consciousness, in spite of their intensity and activity." In explaining the phenomenon of double personality Freud would say that it is a shifting of consciousness, an oscillation between two different psychical complexes which become conscious and unconscious alternately.[18]

But the question still remains: Why does foreconscious activity pass into consciousness with no difficulty, while an unconscious activity is cut off from consciousness? (It is to be noticed here that he no longer speaks of foreconscious and unconscious ideas, but replaces the word idea by the term activity.) In answering this question he says that frequently when we try to represent an idea or a situation to ourselves we become aware of a distinct feeling of repulsion which must be overcome; and when we try to inject such an idea into a patient, we get the signs of what may be called his resistance to it. "So we learn that the unconscious idea is excluded from consciousness by living forces, which oppose themselves to its reception; while they do not object to other ideas,—the foreconscious ones." At the present state of our knowledge, therefore, he suggests the following as the most probable theory that can be formulated: "The unconscious is a regular and inevitable phase in the processes constituting our psychical activity; every psychical act begins as an unconscious one, and it may either remain so, or go on developing into consciousness, according as it meets with resistance or not." Freud illustrates this view by referring to ordinary photography. The first stage of the photograph is the "negative"; every picture has to pass through the negative process; and those negatives which on examination prove to be satisfactory are admitted to the positive process, ending in the picture; those which do not are rejected. Such is the distinction between the foreconscious and unconscious ideas or activities. In reply to his critics that an unconscious idea is inconceivable, he declares that "the existence of an unconscious consciousness is still more objectionable."

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[18] *Ibid.*, p. 315.

Sidis gives three definitions of the subconscious which may be called the medico-popular, the metaphysical and the scientific, respectively. In one place he defines the subconscious "as mental processes of which the individual is not directly aware." In another place he refers to it "as a diffused consciousness below the margin of personal consciousness"; and on a third occasion he defines it "as consciousness below the threshold of attentive personal consciousness."[19]

The subconscious like the conscious may be, according to Sidis, of three types: desultory, synthetic, or recognitive.[20] Sidis would almost banish the term subconscious from literature, and what is commonly called subconscious he would call conscious, while that which is commonly known as the conscious he would call the self-conscious. The self-conscious is that form of mentation which is aware of itself; it is "the knowledge of consciousness within the same moment of consciousness." and in that sense it is identical with personality,[21] On the other hand, the secondary or subconscious self must not be regarded as an individual; "it is only a form of mental life"; it is a coordination of many series of moments-consciousness,"—i. e., pulses of consciousness. And it is these moments-consciousness that are at the heart of the subconscious. Therefore, subconscious experience is not *un*-conscious experience. The proof is this: Normal memory is a reproduction of conscious states. Now, when a subject is hypnotized he can be made to recall an experience which he does not remember in his waking state; and in this he displays memory like normal memory. Therefore, we have proof that his experience was accompanied by consciousness at the time it occurred. Or, to use Sidis's own words, "that in subconscious states there is really present a subconscious consciousness."[22]

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[19] *The Theory of the Unconscious*, p. 319.

[20] *Psychology of Suggestion*, p. 201.

It is to be noticed that this is not the same thing as saying that the ego or the personality was aware of that experience, but on the contrary, there was an awareness of which the attending self had no consciousness.

Having eliminated the subconscious from literature, therefore, there are only two forms of awareness to be considered, according to Sidis: consciousness as such and self-consciousness. The difference between these two states may be made clear in the words of Höffding. "Many feelings and impulses stir within us, without our clearly apprehending their nature and direction. A man who has this feeling does not know what is astir in him; perhaps others see it, or he himself gradually discovers it; but he has the feeling that his conscious life is determined in a particular way."[23] What Höffding means is that there are "mental states of which we have consciousness, but which do not reach the personal consciousness." This is the distinction that Sidis makes between the subconscious and the self-conscious.

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[21] *Ibid.*, p. 198.

[22] *The Theory of the Unconscious*, p. 331.

It naturally follows from the above, and there are many facts in support of the conclusion, that "the stream of subwaking consciousness is broader than that of the waking consciousness, so that the submerged subwaking self knows the life of the upper, primary self, but the latter does not know the former." He admits, however, that there are cases on record showing that the two streams may flow in separate channels; that the two selves may be ignorant of each other.[24]

On the basis of the foregoing view, the phenomenon of double personality is not difficult to explain, thinks the author. When a sufficient number of the submerged moments of consciousness have accumulated they tend to become synthesized, to group themselves in constellations and break forth into attentive consciousness, as do hallucinations, for example. In this manner the secondary consciousness attains self-consciousness, and appears as a new and independent personality. Now and then it "rises to the surface and assumes control over the current of life." This secondary self is aware of and passes judgment on the primary self, while the latter, when it returns, has not the least knowledge of the intruding ego.

It is apparent that the views of Freud and Sidis are essentially the same. The argument, therefore, that exists between these two writers is purely verbal and meaningless. There is no fundamental difference between an unconscious idea and an unconscious moment-consciousness, or even an unconscious consciousness. There may be a difference in quantity but not in quality. Yet we find Freud declaring that if philosophers find it difficult to accept the existence of unconscious ideas, the existence of unconscious consciousness is still more objectionable. To which Sidis retorts: "An idea is essentially of a conscious nature. To speak, therefore, of unconscious ideas is self-contradictory, —it is equivalent to the assumption of an unconscious consciousness."[25] I do not see why Sidis should find fault with this conclusion, since it is the very assumption with which he opens his own discussion on the theory of the subconscious. There he defines the subconscious as mental processes of which the individual is not aware. But what are mental processes if not ideas, images and perceptions? His definition, therefore, turns out to be precisely the same as Freud's.

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[23] Quoted by Sidis in *The Theory of the Unconscious*, p. 339.

[24] *Psychology of Suggestion*, p. 198.

Though the views of neither of these men lend themselves to acceptance in the light of the fundamental postulate of psychology,—namely, that every psychosis has its neurosis (but not the reverse), still Freud's doctrine of the subconscious is somewhat more palatable than that of Sidis. At least it is capable of interpretation in terms of our existing knowledge of neurology; it does not assume too much and does not pretend to offer a solution of all mental phenomena. The view of Sidis, on the other hand, is entirely out of harmony with the fundamental postulate of psychology, and it is so all-embracing and metaphysical in nature as almost to remind one of the teachings of von Hartmann.

This is demonstrated by the vigorous but wholly unjustifiable attack that Sidis launches against the theory of unconscious cerebration. This doctrine, it will be recalled, states that physiological processes may go on in the sensorium which enable the organism to adapt itself to its environment without any consciousness on its part. If this is so, says Sidis, then there is no reason why similar adaptations which are accompanied by consciousness should not also be purely mechanical and automatic. If the writing of letters during somnambulism is automatic, then the correspondence of waking life must be carried on in the same manner. But, he asks, "Can unconscious physiological processes write rational discourse? It is simply wonderful, incomprehensible." Assuming that every sense impression leaves behind it a trace, or a slight modification of nerve tissue, he says, still this does not explain why it is that a series of sensations, ideas, and images experienced at different times "should become combined, brought into a unity, felt. . . . like copies of one original experience."[26] Consequently the subconscious must be considered not as "an unconscious physiological automatism," but as "a secondary consciousness," as a secondary self.[27]

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[25] *The Theory of the Unconscious*, p. 337.

It is doubtful whether the theory of unconscious cerebration can account for the whole of unconscious phenomena, but there is no doubt that Sidis's notion does not account for even a fraction of it. The weakness of his logic is seen in such passages as the following: "Reactions to environment accompanied by intelligence in us are rightly judged to have the same accompaniment in others." From which, of course, he would have us draw the conclusion that since we guide our footsteps on the crowded street, or build a fire, with some degree of waking consciousness or intelligence, therefore the stroller who is absorbed in his newspaper or the somnambulist who builds a fire is also guided by awareness. This conclusion would be correct, provided the proposition on which it is based were not reversible. But it is reversible. It is precisely because we perform many so-called intelligent actions (as judged by their end product) without any consciousness in *our normal* life, that we rightly claim such actions to be devoid of intelligence or active consciousness in *other beings* when performed under the same conditions, or when those beings are abnormal. The above proposition, therefore, stands incomplete without its complement, which says with equal right: Reactions to environment not accompanied by intelligence and attentive consciousness in us are rightly judged to be devoid of these accompaniments in others, especially when those others can give no direct testimony as to the presence of consciousness.

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[26] *Psychology of Suggestion*, p. 125.

[27] *Ibid.*, p. 128.

Let us take an instance of so-called intelligent action which is accompanied without consciousness so far as memory can testify, and see whether it must be explained only on the basis of unconscious-consciousness, or whether a better explanation cannot be found. The case of the person who, though absorbed in his magazine, still picks his way through the crowded thoroughfare will do quite well. Now two wholly unrelated streams of thought cannot occupy the same mind at the same time. To be sure, we may dream and know that we are dreaming, or dream and experience a desire to wake up; or experience both the music and the color effect of an opera at the same time; but these are somewhat related mental complexes: at least they are logically related. We certainly can not solve mathematical problems and at the same time think of our social engagements. Suppose, then, we assume that our hypothetical person is strongly conscious of his reading material only, and is oblivious to the people on the sidewalk. How shall we explain his ability to pick his way through the crowd?

The process may be described thus: Two sorts of stimuli, diverse in nature, impinge on a single sensory organ, the eye. The one stimulus is the words on the printed page, which falls in the center of visual regard; the other stimulus is the people on the sidewalk, perceived in the periphery of vision. Tracing these diverse impressions it seems reasonable to assume that the impression of the printed page is conducted to the occipital lobes, from there to the association centers, and from these the nerve energy is distributed to the other centers, including the motor center, so that when the individual reaches the bottom of the page he makes a conscious and coordinated movement with the hand to turn over a new page. The other vague impressions which fall on the periphery of vision are also conducted to the occipital lobes, but the path to the association centers is already blocked. Naturally the nerve energy seeks an outlet in some other direction. Now in the course of the individual's life, strong association bonds had been formed between visual perceptions of the kind that now impinge on the periphery of his vision and specific organic reactions, i. e., seeing a body coming toward him and moving out of its way. Psychophysically speaking, these strong association bonds are smoothly working conduction-paths between the visual and motor centers. Consequently when now a visual impression of the same kind reaches the visual center, it immediately discharges itself through the path of least resistance, and upon reaching the motor center releases the customary response which, of course, is an adaptation to the external situation. Since all this takes place without reaching the association centers, we have unconscious "intelligent" action.

But it will be asked: How does this view account for the fact that if the individual is hypnotized he can be made to give an account of persons and places he had passed though wholly oblivious of them at the time? The answer to this question involves the physiological theory of the unconscious, and it is to this that we turn next.

Generally stated, this theory means that the subconscious is not psychical at all, but purely physiological; that the presence of awareness cannot be measured by adaptiveness of action, for there are many glands and thousands of cells in the human body performing very complex adaptive acts, or acts designed for the preservation of the organism; yet we do not say that these are mental. Why should we expect less from the tissue of the central nervous system than we do of all other tissue? Or in the words of Münsterberg, "Why cannot they, too, produce physiological processes that yield to well-adjusted results?," i. e., to purposive sensorial excitements and motor impulses.[28]

The same view is advanced by Ribot, who declares that the psychological solution of the unconscious rests on the assumption that consciousness is a quantity which may decrease indefinitely without ever reaching zero. But there is no justification for this postulate; he says: "The results of psychophysists with regard to the threshold of consciousness seem to justify the opposite view, namely, the perceptible minimum appears and disappears instantaneously, and this fact is unfavorable to the hypothesis of an increasing and decreasing continuity of consciousness." The physiologic solution, moreover, is simple, inasmuch as it maintains that subconscious activity is purely cerebral.[29]

The same theory is shared by Jastrow. He deems it a fundamental requisite of any adequate conception of the subconscious that it make a vital connection with normal mental activity; it must find a natural place in an evolutionary interpretation of psychic functions, and like normal activity it must be interpreted in terms of neural dispositions. He proposes a criterion, therefore, for the measure of awareness. "The measure of awareness that shall accrue to any given nervous structure to an environmental situation, in order to render the response advantageous. . . will be determined by the status of the need thus satisfied in the organic life of the individual. The simplest, recur-

[28] *Journ. Abnorm. Psychol.*, II, 1907, p. 30.
[29] *Op. cit.*, p. 35.

rent and constant needs will be sufficiently met by neural dispositions without conscious states, or with the lowest type thereof."[30]

Irving King advances the same view and almost in the same words. "Neural processes," he says, "are accompanied by psychical processes only when there is some need for them."[31] According to him, consciousness is definitely related to the facilitation of reactions and adjustments required by the life process, but which the automatic arrangements of the organism cannot meet. Consciousness either is or is not. It may be more intense at one moment than at another, but it does not consist of different degrees of intensity, as James's theory of the "fringe" would imply. On the neural side, however, we do have a system which may be spatially represented. In terms of this system consciousness is not "the sum of the organization of psychic elements, but rather the unique and single accompaniment of a peculiar organization of neural processes." From this definition it follows that each neural element will determine the complexion of consciousness. If it is in the center of the system, it has dynamic conscious value; if outside of that system, it has potential value only. The subconscious, therefore, is not to be conceived as dim consciousness, but rather as a "physical mass of neural dispositions, tensions and actual processes which are in some degree, perhaps organized, the remnants of habits and experiences, both those which have lapsed from consciousness and those which have never penetrated the central plexus."

On the basis of these definitions it becomes fairly easy to understand most of the phenomena that come under the heads of the conscious and the unconscious. "When consciousness is present," say King, "the neural processes involved are much more intense than otherwise." The dream consciousness is a condition in which the central activity is so subdued that more or less fragmentary neural dispositions are aroused. In hypnosis, again, the center of activity is shifted in more or less degree, resulting in the temporary lapse from consciousness of some processes and the incorporation of others which were previously mere neural dispositions. While in multiple personality there are one or more strongly organized potential systems of neural elements which, under appropriate conditions, can separately become sufficiently active to be conscious.[32]

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[30] *The Unconscious*, p. 411.

[31] *Op. cit.*, p. 42.

It is to be noted that the chief characteristic of the exponents of the physiologic theory is that they do not endow the subconscious with any mysterious powers, they do not regard it as the reservoir of consciousness, but on the contrary, they consider subconscious events as very much like the ordinary facts of waking consciousness; and their method of explanation is to proceed in a true scientific manner from the known to the unknown, from the facts of the conscious to those of the unconscious. And although Morton Prince does not hold this view in its entirety, it is nevertheless in this fashion that he commences the presentation of what is without doubt the most able and most cogent theory of the unconscious that has appeared in recent years.

The problem of the subconscious, according to him, is the problem of memory. Whoever solves the latter will also have solved the former. Memory should be considered from two points of view: as a process and as an end result. As a process it is composed of three factors,—registration, conservation and reproduction. The last is the end result, but to understand this we must know something of, or at least have a plausible theory concerning, impression and conservation.

Instances of the conservation of forgotten experiences abound both in normal and pathological life. They are such as lapses of memory, forgotten acts, failure to recognize, or in abnormal cases they become manifest in automatic writing and speech, in post-hypnotic suggestions, and so forth. After examining the facts in great detail, Prince comes to the conclusion that it does not matter at what period of life or in what state experiences have occurred, "or how long a time has intervened since their occurrence, they may still be conserved. They become dormant, but under favorable conditions, they may be awakened and may enter conscious life."[33] Naturally these experiences must be conserved in some form; and whatever the nature of this form may be it is obvious that the experiences themselves must have "a very specific and independent existence, somewhere and somehow, outside of the awareness of consciousness."

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[32] *Ibid.*, pp. 45ff.

Now in order to account for normal memory we must posit that ideas which have passed through the mind have been conserved through some residuum left by the original experience. This residuum must be either psychological or physiological. Suppose we consider the former alternative first. We shall have to assume that sensations, perceptions, emotions and even complex systems of ideas are capable of pursuing "autonomous and contemporaneous activity outside of the various systems of ideas that make up the personal consciousness." This is an untenable view, for it would necessitate the storing up of millions of ideas and infinite forms of associations. Let us, therefore, consider the other alternative, namely, conservation as physical residua. This view is based on the assumption that whenever we have a mental experience of any sort some change or trace is left in the neurones of the brain. This does not necessarily mean that the neural modification is the cause of the conscious process. On the contrary, it assumes the postulate of psychophysical parallelism and declares that with every passing state of conscious experience, with every idea, emotion and perception, the brain process that is functioning leaves some trace, some residua of itself within the neurones and in the functional arrangements among them. This physiological conception is at the basis of the association theory, wherein it is assumed "that whenever a number of neurones involved in a coordinated sensory-motor act are stimulated into functional activity, they become so associated and the paths between them become so opened or sensitized, that a disposition becomes established for the whole group to function together and to reproduce the original reaction when either one or the other is afterward stimulated into activity. This 'disposition' is spoken of in physiological language as a lowering of the threshold of excitability. This change we may speak of as a residuum,"[34] says Prince.

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[33] *The Unconscious*, pp. 82ff.

We are now in a position to answer the question raised a while ago concerning the ability of a hypnotized person to recall a forgotten experience or one that he was not aware of at the time of its occurrence.

The neurones in retaining the residua of the original process have become organized into a functioning system corresponding to the system of mental states—whether ideas, perceptions or emotions—which accompanied that original experience and are now capable of reproducing it. Hence when we reproduce the original ideas in the form of memory it is because there is a refunctioning of the physiological neural process. On hypnotizing a person, therefore, and asking him to recall a forgotten event, we simply start that process by introducing what may be called a catalytic agent, i. e., we stir one neurone or one brain cell, or one part of the system, and that sets the entire system working precisely as it did on the original occasion. This physiological functioning now reaches consciousness or motor expression, because all other mental processes are arrested for the time being, thus facilitating a greater discharge of nerve energy in this one direction.

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[34] *Ibid.*, pp. 119-120.

The same is true of crystal gazing and automatic writing. In the former occurrence there is an intense concentration of primary attention. That is, the subject does not attend to any idea or to a situation from which he tries to derive meaning, but merely to a visual stimulus. In this manner all distracting influences and mental processes which do not harmonize with the original experience, of which it must be said the individual has some intimation to begin with, are arrested. Thus the resumption of the original neural process is facilitated and with it, of course, the psychical accompaniment. Anything that will hold the attention will do as well as a crystal. A soft light will work just as well.

Equally well can automatic writing be explained on the basis of this theory. The writing habit is very highly and delicately "developed in us writing mortals," to use a phrase of Pierce, and it is no wonder that it may operate mechanically, when for some reason its neural system has become detached from that other system which constitutes self-consciousness. Nor do the specimens of automatic writing show this phenomenon to be essentially different from the uncontrolled movements of the hands and bodily twitching that most of us have at times; and by no means is it different from such nervous troubles as chorea and locomotor ataxia. The hand has been observed to write backwards, to write mirror script, to follow indefinitely a direction given to it by the experimenter such as moving in a circle, it misplaces and omits letters. "Surely," says Pierce, "such occurrences point clearly to a disordered neural mechanism, rather than to a perverse or humorously inclined secondary consciousness."[85]

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[85] Carman, *Studies in Phil. and Psychol.*, 1906, pp. 327-328.

We see, then, that most if not all subconscious phenomena can best be explained in terms of cerebration. Now it is necessary to have some term to designate the separate neurological modifications, and Prince calls these "neurograms." A neurogram, therefore, is a brain record; and, just as a phonogram characterizes the form in which the physical aspect of spoken thought is recorded, so a neurogram characterizes the form in which thoughts and other mental experiences are recorded in the brain tissue. Of course this is merely a theoretical concept, like atoms and moments of force.

Though memory is regarded in psychology as a conscious process, it is evident that on the basis of the foregoing view any process that consists of the three factors, registration, conservation and reproduction of experiences, must be considered as memory, "whether the final result be the production of a conscious experience or of one to which no consciousness was ever attached."[36]

That memory is ultimately a physiological phenomenon was demonstrated by the experiments of Rothmann who showed that decorticated animals can be educated, i. e., new dispositions and new associations may be established in the lower centers "without the intervention of the integrating influence of the cortex or conscious intelligence."[37] The bearing of this fact is that unconscious processes are capable of being conserved in the form of physiological memory.

If we accept the psychophysiological theory of memory, then, we may define the unconscious as the brain residua, the physiological dispositions or neurograms in which the experiences of life are conserved. The co-conscious, on the other hand, means "a coexisting consciousness of which the personal consciousness is not aware. And since these two function together we need an inclusive term, one that will embrace them both, and that is the subconscious."[38]

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[36] Prince, *The Unconscious*, p. 135.

[37] *The Unconscious*, p. 238.

Here the truly scientific discussion ends. The rest that Prince has to say about the subconscious is metaphysical, and not unlike the views of Sidis, von Hartmann and Myers. He declares, for instance, that the subconscious, rather than the conscious, is the important factor in personality and intelligence; that the subconscious furnishes the material out of which our judgments and beliefs, our ideals and characters, are shaped. Yet I can hardly see how he squares this statement with the next in which he says that the unconscious complexes are kept in check by the normal inhibitions and the counterbalancing influences of the normal mental mechanism.[39] Evidently, then, it is the normal mental mechanism, by which I suppose he means attentive consciousness or intelligence, which exercises a determining control over the unconscious complexes. Hence it is the conscious and not the unconscious which is at the basis of our beliefs, our ideals and character.

Be this as it may, Prince's metaphysical interpretation does not change the facts nor the value of his scientific concepts which so excellently explain those facts. For by resolving the subconscious into unconscious physiological dispositions on the one hand, and coactive conscious states on the other, we are able to understand more clearly the nature of lapsed memory, absent-mindedness, post-hypnotic suggestion, artificial hallucinations, hysteria, psychoneurosis and multiple personality.

With respect to bridging the gap between the conscious and the subconscious, Prince declares that no gap exists. What belongs to one at times passes into the other, and *vice versa.* Consciousness may be conceived of as a round disk with attention or the focus of awareness at the center.

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[38] *Ibid.,* p. 253.

[39] *Ibid.,* p. 262.

Surrounding this is a zone which constitutes the fringe of awareness. Embracing that is the co-conscious, i. e., unconscious mentation, while the outermost zone comprises the unconscious processes. There is a gradual shading from the center to the edge of this figurative disk or sphere of consciousness. But here again Prince treads on the metaphysical, and we have not the time to follow him.

The space at our disposal only permits us to suggest that more original work ought to be done in this field. Too many writers weave their theories around the same cases of somnambulism and double personality. The cases examined by Morton Prince, Janet and Bernheim constitute a sort of stock-in-trade making their rounds in the literature on the subconscious. But a theory does not gain credence by hopping about on the same crutches; it must gather new facts if it would increase in strength. In this respect Professor Lillien Martin has shown a good way in her experimental investigation of the subconscious. She does not add anything new, but her method of investigation which consisted in having normal subjects permit images to arise of themselves and then introspect on them, is more reliable than the questionnaire method used by some authors, or the observation of pathological cases upon which still others have built their concepts. Experimental research under strictly controlled conditions, should be the slogan of psychologists in the field of the subconscious as it is in that of the conscious.

Thus we stand at the present moment with three theories of the unconscious before us. The psychometaphysical, the psychophysiological with metaphysical leanings, and the psychoneurological with scientific leanings. The first declares that the whole universe is permeated with consciousness, that there is intelligence in all animals, in plants and even in inorganic matter. This notion is held by writers like von Hartmann, Myers, Delboeuf and persons interested in psychical research. It is obvious, however, that this view will not bring us anywhere.

The psychophysiological theory with metaphysical leanings also maintains that there is consciousness in all organisms, only it is not conscious of itself. That in living organisms this consciousness is accompanied by physiological changes, but these changes are not necessarily the cause of conscious phenomena. Neurological modifications are only conceptions assumed for the purpose of explaining unconscious activity. But the psyche is the fundamental principle. The unconscious is the source of all intelligence. This view is held explicitly or implicitly by Freud, Sidis, Prince, Lloyd Morgan and Janet.

Finally, the psychophysiological theory with scientific leanings asserts that neurological modifications are the essential factors of conscious and unconscious phenomena. That consciousness appears only when the neurones attain a certain tension, or function in a certain relation; that consciousness may or may not accompany so-called intelligent actions performed under pathological conditions; that it is certainly not present in instinctive functioning which characterizes the life of lower animals; that the unconscious is not the storehouse of the conscious, that there is nothing mysterious or wonderful about it, and that with further investigation its precise nature and place in the scale of psychogenesis will be at the command of psychologists. This view is held by writers like Ribot, Pierce, King and Jastrow.

Such are the three views that present themselves for our consideration. There is no doubt about the one that scientists will adopt as leading to a greater extension of human knowledge.

GUSTAVE A. FEINGOLD.

HARTFORD, CONN.

NIRVANA

THE BUDDHIST'S FINAL GOAL.

NIRVANA, state of rest unbroken, where
Benign extinction of all passion rules—
A rest so deep that in eternity
It shall not be disturbed—I long for thee!
After life's storm and stress thou grantest peace.
Weary of this world's wild anxieties,
Its pains and empty pleasures, I will seek
The everlasting in blank vacancy,
Thus to attain the boon of dreamless sleep
From which nor rancor of a villainous
Intrigue, planned by malevolence or hate,
Nor the misfortune of a sorry slip
Of my misguided feet, will waken me,
But unconcerned and calm I shall remain
In perfect quietude. For I'll be safe
From all the worry and from all the trouble
That restlessly stirs life and keeps it moving.
The bustle of the world, its vulgar noise
With its deplorable afflictions, trials
And eke malicious slander, will be hushed.

There is a refuge, vainly sought for here,
And in its sanctuary I'll find shelter
From life's great woes and small annoyances.
There paltry problems will no longer vex
Nor will demand immediate solution.

I shall no longer be disquieted
By urgent needs to be responded to
In energetic action. E'en my ego
With its ambitions, wants and vanities;
Its recollection of the past with all
Its sweet and bitter memories—all that,
My very consciousness, will be extinct.
I shall be left in tranquil emptiness
And in a soothing void of non-existence,
A clean, pure state of rest most absolute,
Without the slightest ripple of disturbance,
A panacea for all earthly ills,
An anodyne for any pang or pain.

In former ages mankind felt assured
Of a survival of the soul. The savage
Met his dead parents and his friends in dream.
He saw them, he conversed with them, and dreams
Were real to him just as actual life.
When man grew wiser, he began to doubt
And he grew anxious for convincing proof.
Though proofs were negative, yet he still clung
To hope expressing his desire to live
And to prolong his life beyond the grave.

Oh foolish man, why dost thou shrink from death
And yearnest greedily for prolongation
Of thy ill-favored self? Thy selfishness
Thou wishest to preserve, thy abject foibles,
Instead of gladly hiding them into
The darkness of a taciturn forgetting,
As in wise justice Nature has intended,
Thou wouldst perpetuate with petulance
And peevish childishness that of thyself,
Exactly that, whose riddance should be welcomed

As a great boon, a seemly liberation
From slavery, its drudgery and curse.
Why should we cling to chains that burden us
When we might cast them off and free ourselves?
Why should we serve new terms as sentenced convicts
When duly our acquittal is pronounced
And a reprieve has graciously been granted?

Mara, the Evil One will envy me
In my benign repose; he will continue
His vicious persecution. Shall I help him
And do the wrong myself unto myself
By pressing from a place of safety into
My prison with its ugly bars? Oh no!
No, I shall not! For I prefer my freedom!
There I shall be where most malignant foes
Shall not be able to do any harm.
And if they should go on abusing me
I shall no longer heed their defamation;
I'll leave them to their fate which in full justice
Will come to them without my interference.
Their lies no longer touch me. In Nirvana
I shall be free; the vicious will remain
In a gehenna builded by themselves.

The wild desires of my hot pulsing heart
Will then be calmed, all hunger will be stilled,
All thirst be quenched in deepest satisfaction.
And mine shall be the glory of Nirvana;
Having achieved the conquest of all pain,
Having attained final emancipation,
It will be mine, Nirvana will be mine.
I shall be free when I have closed mine eyes,
To enter death, life's solemn grand finale,
Its fruitage, benison and consummation.
Nirvana's holy peace shall then be mine.

Indeed it is mine here; I live it now
If I but understand the art of living
The truth: It is and will be mine, when I
Surrender transient things to transiency
And live in that alone which will endure.

Oh, the inanities of self! how puny,
How paltry are they; and how kind is death
To brush them off with gently sweeping stroke
Like spider webs out of a gloomy corner,
Together with the spider who has built them.

O let them go without regret and sorrow,
The ego with its portion is not worthy
Of preservation. It is but the burden
Of our existence, the receptacle
In which the weaknesses and faults of life
Are bred, in which its plagues are caught and stored.
So let them go and bless their disappearance.
They are like painful sores that should be healed,
And when our ego passes they are cured.
The right ideas only which we've thought,
The good deeds too which we have done and things
Of beauty we have shaped, they shall survive.
They are our better selves; they will be helpful,
Helpful to others, to the generations
That are to come, helpful like gifts of God,
Like rain or sunbeams, showered down on earth,
Profuse, unstinted, and with utter lack
Of egotism. But do not cling to self,
Nor yearn for any undue preservation
Of personality. Our ego's life
And all that's of an accidental nature
Be handed over to its destination
Which is a dissolution into naught.

Our conscious ego has originated,
It has been growing, and 'twill pass away.
Such is its destiny and so 'tis best.
But I will glory in my future lot—
Nirvana's boon, the state of perfect peace.

Yea, I can enter even now into
Nirvana's hallowed temple where my soul
Is liberated from all transiency
And will be ready for a final exit
Out of existence with its narrowness
Into the better and superior realm,
The realm of bliss, Nirvana's noble bliss.

Praised be Nirvana, glorious radiant state
Of biding peace, hope of all living creatures
And comfort of the dead. Holy asylum
Which grander is than highest joy in heaven
And more divine than the divinity
Of Brahma and his gods in all their splendor.
Praised be Nirvana, goal of all the Buddhas!
And blest is he who enters there, who lives
There in Nirvana; lives there in the truth
Which therein is revealed; he who is free
From vain attachment, who's above temptation.
'Tis he in whom all passion is extinct;
Who has attained life's final aim Nirvana,
Goal of the wise, and of the blessèd Buddhas.
He who has reached it is the Conqueror,
The conqueror of Evil, the great Jina,
He's the Enlightened One: he is the Buddha!
And he is blest; the Buddha, yea! is blest.
Pathfinder to Nirvana! Praised be he!
Namo tassa Bhagavato Buddhassa.

PAUL CARUS.

THE MANUSCRIPTS OF LEIBNIZ ON HIS DISCOVERY OF THE DIFFERENTIAL CALCULUS.*

PART II.

§ III.

The following notes, on certain MSS. which Gerhardt does not give in full, are taken from G. 1848, p. 20 et seq. (see also G. 1855, p. 55 et seq.)

In a manuscript of August, 1673, bearing the title *Methodus nova investigandi Tangentes linearum curvarum ex datis applicatis, vel contra Applicatis ex datis productis, reductis, tangentibus, perpendicularibus, secantibus,* Leibniz begins at once with an attempt to find a method that is applicable to any curve for the determination of its tangent. "But if," says Leibniz with regard to the classification of curves which Descartes laid down as fundamental for his method of tangents, "the figure is not geometrical — such as the cycloid—it does not matter; for it will be treated as an example of a geometrical curve, by supposing that there is a relation between the straight lines and curves by which they are made known to us; in this way, tangents can be drawn just as well to either geometrical or ageometrical curves, as far as the nature of the figure allows." He considers the curve as a polygon with an infinite number of sides, and here already he constructs what he calls the "Characteristic Triangle," whose sides are an infinitely small arc of the curve, and the differences between the ordinates and between the abscissae; this is similar to the triangle whose sides are the tangent, the subtangent and the ordinate for the point of contact. In just the same manner as used by Descartes, Leibniz seeks the tangent by means of the subtangent; he denotes the infinitely small differences of the abscissae by *b*, and verifies for the parabola, that his method works out correctly, when the terms of the equation that contain the infinitely small quantities are neglected. The omission of these terms, however, does not appear to Leibniz to be a method to be relied upon. In fact, he says: "It is not safe to reject multiples of the infinitely small part *b*, and other things; for it may happen that through the compensation of these with others,[1] the equation may come to a totally different condition." So he seeks to obtain the determination of the subtangent in some other way. "The whole question is, how the applied lines can be found from the differences of two applied lines," are his own words. He then finds that the solution of this problem reduces to the summation of a series, of which the terms are the differences of consecutive abscissae.

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* Part I appeared in *The Monist* of October, 1916.

At the end of the manuscript Leibniz proceeds to speak of the inverse problem: "It is an important subject for investigation, whether it is possible, by retracing our steps, to proceed from tangents and other functions to ordinates. The matter will be most accurately investigated by tables[2] of equations; in this way we may find out in how many ways some one equation may be produced from others, and from that which of them should be chosen in any case. This is, as it were, an analysis of the analysis itself, but if that is done it forms the fundamental of human science, as far as this kind of things is concerned." Ultimately Leibniz obtains the following result: "The two questions, the first that of finding the description of the curve from its elements, the second that of finding the figure from the given differences, both reduce to the same thing. From this fact it can be taken that almost the whole of the theory of the inverse method of tangents is reducible to quadratures."

According to this, Leibniz has in the middle of the year 1673 already attained to the knowledge that the direct and the so-called inverse tangent-problem have an undoubted connection with one another; he has an idea that the latter may be capable of reduction to a quadrature (i. e., to a summation).

Again, in a manuscript dated October 1674, i. e., fourteen months later, which bears the title *Schediasma de Methodo Tan-*

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[1] It is impossible to see, without a fuller knowledge of the context, whether this refers to "compensation of errors," or whether Leibniz is alluding to the possibility of all the finite terms cancelling one another.

[2] Leibniz comes back to this point later; see § IV.

gentium inversa ad circulum applicata, he is able to say for certain that "the quadratures of all figures follow from the inverse method of tangents, and thus the whole science of sums and quadratures can be reduced to analysis, a thing that nobody even had any hopes of before."

After Leibniz thus recognized the identity between the inverse tangent-problem, of which the general solution had not been found by Descartes, and the quadrature of curves, he applied himself to the investigation of series by the summation of which quadratures were then obtained. In a very extensive discussion, bearing the date of October, 1674, and the title *Schediasma de serierum summis, et seriebus quadraticibus,* Leibniz starts from the series

$$\frac{b}{1}-\frac{b^2}{2}+\frac{b^3}{3}-\frac{b^4}{4}+\frac{b^5}{5}-\ldots\ldots,$$

and obtains the following general rule: "By calling the variable ordinates x, and the variable abscissae y, and b the abscissa of the greatest ordinate e, and d the abscissa of the least ordinate h," are Leibniz's own words, "we have the following rules:

$$\frac{x^2}{2}=ywx-\frac{yw^2}{2}+\frac{d^2h}{2},$$

$$\frac{h^2w}{2}+\frac{d^2h}{2}=xy-\frac{x}{2},\ e-h=w,$$

$$xw=\frac{e^2}{2}-\frac{w^2}{2},$$

$yw=x$ in decreasing values, for in ascending or increasing values $yw=eb-x$."[8]

Leibniz then goes on to remark: "These rules are to be altered slightly according as the series increase or decrease; also mention of the least ordinate may be omitted, if it is always understood to be the last ordinate; on the other hand, w can always be inserted wherever mention is made of w. All series hitherto found are contained in the one by means of these rules, except the series of powers, which is to be obtained by taking differences."

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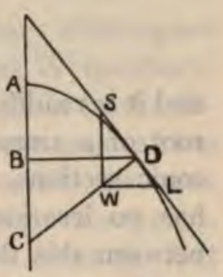
[8] This, without either proof or figure, is a hopeless muddle; and yet it is repeated word for word, without any addition or remark, in Gerhardt's 1855 publication. Goodness knows what the use of it was supposed to be in this form! Unless Leibniz has omitted some length, which he has supposed to be unity, the dimensions are all wrong.

In the same essay, Leibniz makes use of a theorem, which he has probably found to be general at an earlier date, namely:

"Since BC is to BD as WL to SW, therefore BC⌒SW,[4] that is, the sum of every BC [applied to AC], is equal to BD⌒WL, that is, the sum of every BD applied to the base; moreover, the sum of every BD applied to the base is equal to half the square on the greatest BD. Further, it is evident that the sum of every WL is equal to the greatest BD."



Accordingly, Leibniz comes to the further conclusion that the method of Descartes, which uses a subsidiary equation with two equal roots, to solve the general inverse-tangent problem, is unsatisfactory. In a manuscript of January, 1675, Leibniz says: "Thus at last I am free from the unprofitable hope of finding sums of series and quadratures of figures by means of a pair of equal roots, and I have discovered the reason why this argument cannot be used; this has worried me for quite long enough."[5]

§ IV.

The manuscript that comes next in date is one that is given in G. 1855. It really consists of three short notes, (1) a theorem on moments, (2) a continuation of the idea started at the end of the manuscript of August, 1673 (§ III), namely the formation of tables of equations that are derivable from certain standard equations, with the appropriate substitutions for each case, (3) a return to the consideration of moments.

This is the first appearance of the word "moment," but from the context it is evident that Leibniz has done some considerable amount of work upon the idea before. If the theorem that is first given is written in modern notation, it takes the form of an "integration by parts" and serves to change the independent variable. Thus we have

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[4] The sign ⌒ signifies multiplication.

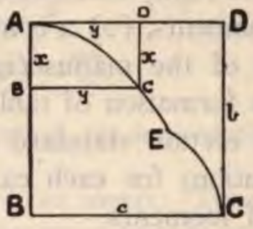
[5] Observe that as yet nothing has been said about the area of surfaces of revolution or moments about the axis, although we should expect them to be mentioned in connection with the figure that is given; for the next manuscript shows that in October 1675, Leibniz has already done a considerable amount of work on moments.

$$\int xy\,dx = \left[\frac{x^2y}{2}\right] - \int \frac{x^2}{2}\,dy;$$

and it is readily seen that if x can be expressed as a square root of a simple function of y, as for the circle and the conic sections, then the integral on the right-hand side has no irrationality. This, I take it, is the connection between this theorem and those which follow.

The proof is not so clear as it might be on account of two errors, both I think errors of transcription or misprints. The first *a* should be an x, and the second *a* should be the preposition *a* (= from); also, for modern readers the figure might be improved by showing the *variable* lines AB (= x), BC (= y) as in the accompanying diagram. The argument then is as follows:

Moment of BC(= y) about AD is xy, when it is applied to AB for the summation; for this brings in the infinitesimal breadth of the line.



Moment of DE (= x) about AD is $x^2/2$, when applied to AD, so as to include the infinitesimal breadth of the line, and assuming that the line may be considered to be condensed at its center of gravity. The theorem follows at once.

Note the use of the sign ⊓ as a symbol of equality, which I have allowed to stand in the opening paragraph. Leibniz adopts the ordinary sign two months later, or Gerhardt makes the change,[6] so I have not thought it necessary to adhere to it, but only to show it in the opening paragraph.

The only remark that seems to be necessary with regard to the second part of this manuscript is that Weissenborn[7] argues from the continued allusion by Leibniz to the desirability of forming tables of curves whose quadratures may be derived from those of others, especially the conic sections, (starting with the manuscript of November, 1675, where Weissenborn states that it is first hinted), that Leibniz had probably either seen or heard of the *Catalogus curvarum ad conicas sectiones relatarum* of Newton. The point is that Weissenborn seems to have missed the clear reference to the reduction of curves to those of the second degree, in this manuscript of October, 1675. It may of course be just possible that G. 1855, in which this MS. appears, was not at Weissenborn's hand at the time that he wrote, for Weissenborn's book was published in 1856.

With regard to the third part, it will be found in the original Latin that Leibniz, after apparently starting with perfect clearness, gets rather into a muddle toward the end. This is however only apparent, being partly due to an inaccurate figure, and partly to what I am convinced is an error of transcription. This incorrect sentence makes Leibniz write apparently absolute nonsense; but if a correction is made according to the suggestion in the footnote, and reference is made to the corrected diagram that I have added on the right of the figure of Leibniz, as given by Gerhardt, then the proof given by Leibniz reads perfectly smoothly and sensibly.

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[6] Gerhardt has a footnote to the effect that, as nearly as possible he has retained the exact form of this and the manuscripts that immediately follow; except in the matter of this one sign I have adhered to the form given by Leibniz.

[7] Weissenborn, *Principien der höheren Analysis,* Halle, 1856.

25 October, 1675.

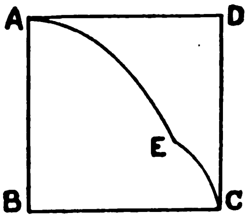
Analysis Tetragonistica Ex Centrobarycis.

Analytical quadrature by means of centers of gravity.

Let any curve AEC be referred to a right angle BAD; let AB $\sqcap$ DC $\sqcap a$,[8] and let the last $x \sqcap b$; also let BC $\sqcap$ AD $\sqcap y$, and the last $y \sqcap c$. Then it is plain that

$$\text{omn.}\ \overline{yx \text{ to } x} = \frac{b^2c}{2} - \text{omn.}\ \overline{\frac{x}{2} \text{ to } y}. \cdots\cdots\cdots\cdots\cdots\cdots (1)$$

For, the moment of the space ABCEA about AD is made up of rectangles contained by BC ($= y$) and AB ($= x$); also the moment



about AD of the space ADCEA, the complement of the former is made up of the sum of the squares on DC halved $\left(= \frac{x^2}{2}\right)$; and if this moment is taken away from the whole moment of the rectangle ABCD about AD, i. e., from c into omn. x,[9] or from $\frac{b^2c}{2}$, there will remain the moment of the space ABCEA. Hence the equation that I gave is obtained; and, by rearranging it, it follows that

$$\text{omn.}\ yx \text{ to} \cdot x + \text{omn.} \frac{x^2}{2} \text{ to } y = \frac{b^2c}{2} \ \cdots\cdots\cdots\cdots\cdots\cdots (2)$$

In this way we obtain the quadrature of the two joined in one in every case; and this is the fundamental theorem in the center of gravity method.

Let the equation expressing the nature of the curve be

$$ay^2 + bx^2 + cxy + dx + ey + f = 0, \cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots (3)$$

and suppose that $xy = z, \cdots\cdots (4)$, then $y = \frac{z}{x}. \cdots\cdots\cdots\cdots (5)$

Substituting this value in equation (3), we have

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[8] This a should be x.

[9] Here, in the Latin, "ac in omn.x" should be "a c in omn.x."

$$\frac{az^2}{x^2}+bx^2+cz+dx+\frac{ez}{x}+f=0, \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots (6)$$

and, on removing the fractions,

$$az^2+bx^4+cx^2z+dx^3+exz+fx^2=0. \quad\ldots\ldots\ldots\ldots\ldots (7)$$

Again, let $x^2=2w$ ....(8); then, substituting this value in equation (3), we have

$$ay^2+2bw+cxy+dx+ey+f=0, \quad\ldots\ldots\ldots\ldots\ldots\ldots (9)$$

and therefore

$$x=\frac{-ay^2-2bw-ey-f}{cy+d}, \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots (10)$$

$$=\sqrt{2w}; \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (11)$$

and, squaring each side, we have[10]

$$\begin{aligned}a^2y^2+4aby^2w+2aey^3+2afy^2+4b^2w^2+4bewy+4bfw\\ +e^2y^2+2fey+f^2-2c^2y^2w-4cdyw-2d^2w=0. \quad ..(12)\end{aligned}$$

Now, if a curve is described according to equation (7), and also another according to equation (12), I say that the quadrature of the figure of the one will depend on the quadrature of the figure of the other, and *vice versa.*

If, however, in place of equation (3), we took another of higher degree, the third say, we should again have two equations in place of (7) and (12); and continuing in this manner, there is no doubt that a certain definite progression of equations (7) and (12) would be obtained, so that without calculation it could be continued to infinity without much trouble. Moreover, from one given equation to any curve, all others can be expressed by a general form, and from these the most convenient can be selected.

If we are given the moment of any figure about any two straight lines, and also the area of the figure, then we have its center of gravity. Also, given the center of gravity of any figure (or line) and its magnitude, then we have its moment about any line whatever. So also, given the magnitude of a figure, and its moments about any two given straight lines, we have its moment about any straight line. Hence also we can get many quadratures from a few given ones. Moreover, the moment of any figure about any straight line can be expressed by a general calculation.

The moment divided by the magnitude gives the distance of the center of gravity from the axis of libration.

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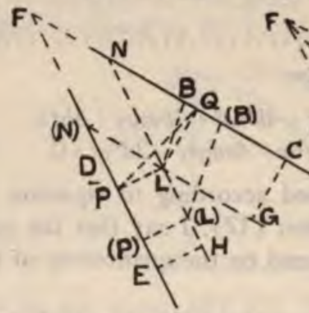
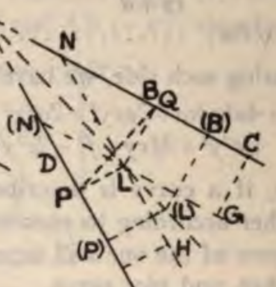
[10] In view of this accurate bit of algebra, the faulty work in subsequent manuscripts seems very unaccountable.

Suppose then that there are two straight lines in a plane, given in position, and let them either be parallel or meet, when produced in F. Suppose that the moment about BC is found to be equal to ba^2, and the moment about DE is found to be ca^2. Call the area of the figure v; then the distance of the center of gravity from the straight line BC, namely CG, is equal to $\frac{ba^2}{v}$, and its distance from the straight line DE, namely EH, is equal to $\frac{ca^2}{v}$; therefore CG is to EH as b is to c, or they are in a given ratio.[11]



GERHARDT'S DIAGRAM.



SUGGESTED CORRECTION.

Now suppose that the straight line EH, remaining in the plane, traverses the straight line DE, always being perpendicular to it, and that the straight line CG traverses the straight line BC, always perpendicular to it, and that the end G leaves as it were its trace, the straight line G(N), and the end H the straight HN. Then, if BC and DE meet anywhere, G(N) and HN must also meet somewhere, either within or without the angle at F. Let them meet at L; then the angle HLG is equal to the angle EFC, and PLQ (supposing that PL = EH and LQ = CG) will be the supplement of the angle EFC between the two straight lines, and will thus be a given angle. If then PQ is joined, the triangle PQL is obtained, having a given vertical angle, and the ratio of the sides forming the vertex, QL : LP, also given.

When then BL is taken, or (B)(L), of any length whatever, since the angle BLP always remains the same, and in addition we have BL to LP as (B)(L) to (L)(P), therefore also BL to (B)(L) as LP to (L)(P); and this plainly happens when FL is also proportional to these, that is, when a straight line passes through F, L, (L),......

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[11] This proves the fundamental theorem given lower down, with regard to a pair of parallel straight lines; and he now goes on to discuss the case of non-parallel straight lines.

Hence, since we are not here given several regions, it follows that the locus is a straight line. Therefore, given the two moments of a figure about two straight lines that are not parallel,........., the area of the figure will be given, and also its center of gravity.[12]

Behold then the fundamental theorem on centers of gravity. If two moments of the same figure about two parallel straight lines are given, then the area of the figure is given, but not its center of gravity.

Since it is the aim of the center of gravity method to find dimensions from given moments, we have hence two general theorems:

If we are given two moments of the same figure about two straight lines, or axes of libration, that are parallel to one another, then its magnitude is given; also when the moments about three non-parallel straight lines are given. From this it is seen that a method for finding elliptic and hyperbolic curves from given quadratures of the circle and the hyperbola is evident.[13] But of this in a special note.

§ V.

The next manuscript to be considered is a continuation of the preceding, and is dated the next day. Its character is of the nature of disjointed notes, set down for further consideration.

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[12] The passage in Gerhardt reads:

Datis ergo duobus momentis figurae ex duabus rectis non parallelis, dabitur figurae momentis tribus axibus librationis, qui non sint omnes paralleli inter se, dabitur figurae area, et centrum gravitatis.

For this I suggest:

Datis ergo *tribus* momentis figurae ex *tribus* rectis non parallelis, *aliter* figurae momentis tribus axibus librationis, qui non *sunt* omnes paralleli inter se....

The passage would then read:

Given three moments of a figure about three straight lines that are not parallel, in other words, the moments of the figure about three axes of libration, which are not all parallel to one another, then the area of the figure will be given and also the center of gravity.

If the alternative words are *written* down, one under the other, and not too carefully, I think the suggested corrections will appear to be reasonable.

[13] Apparently, here Leibniz is referring back to the theorem at the beginning of the article.

26 October, 1675.

Another tetragonistic analysis can be obtained by the aid of curves. Thus, let the same curve be resolved into different elements, according as the ordinates are referred to different straight lines. Hence also arise diverse plane figures, consisting of elements similar to the given curve; and since all of these are to be found from the given dimension of the curve, it follows that from the dimension of any one of the curves of this kind the rest are obtained.

In other ways it is possible to obtain curves that depend on others, if to the given curve are added the ordinates of figures of which the quadrature is either known or can be obtained from the quadrature of the given one.

Just as areas are more easily dealt with than curves, because they can be cut up and resolved in more ways, so solids are more manageable than planes and surfaces in general. Therefore, whenever we divert the method for investigating surfaces to the consideration of solids, we discover many new properties; and often we may give demonstrations for surfaces by means of solids when they are with difficulty obtained from the surfaces themselves. Tschirnhaus observed in a delightful manner that most of the proofs given by Archimedes, such as the quadrature of the parabola, and dependent theorems on the sphere, cone, and cylinder, can be reduced to sections of rectilinear solids only, and to a composition that is easily seen and readily handled.

Various ways of describing new solids.

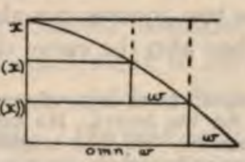
If from a point above a plane a rigid descending straight line is moved round an area, of any shape whatever, diverse kinds of conical bodies are produced. Thus if the plane area is bounded by the circumference of a circle, a right or scalene cone is produced. Also if the figure used for the base, or the plane area, has a center—an ellipse for example—then we get an elliptic cone, which is a right cone if the given point is directly above the center, and if not it is scalene. Another conic gives another elliptic cone.

If the rigid line drawn down from the point is circular or some other curve, at one time it is so fixed to the point or pole that it has freedom to move in one way only, say round an axis, in which case it is necessary that the base should be a circle and that the fixed point or pole should be directly over the center. At another time it is necessary that the rigid line should have freedom for other motions, such as an up and down motion, or some other motion, controlled by some straight line; and then it will always ascend or descend when necessary, so that it ever touches the given plane area by its rotation round the axis; and this is the second class of cones. A third class consists of those in which, besides the double motion of a rotation round an axis and an up and down motion, the curve alone, or the axis alone, or even both the curve and the axis, also perform other motions meanwhile, or even the point itself moves.

Here is another consideration.

The moments of the differences about a straight line perpendicular to the axis are equal to the complement of the sum of the terms; and the moments of the terms are equal to the complement of the sum of the sums, i. e.,

$$\text{omn.}\overline{xw} \sqcap \text{ult.}x,\ \overline{\text{omn.}w},, - \overline{\text{omn.}\ \overline{\text{omn.}w}} \qquad (14)$$



Let $xw \sqcap az$, then $w \sqcap \frac{az}{x}$, and we have

$$\text{omn.}az \sqcap \text{ult.}x,\ \text{omn.}\frac{az}{x} - \text{omn.omn.}\frac{az}{x}\ ;$$

hence $$\text{omn.}\frac{az}{x} \sqcap \text{ult.}x\ \ \text{omn.}\frac{az}{x^2} - \text{omn.omn.}\frac{az}{x^2}\ ;$$

inserting this value in the preceding equation, we have

$$\text{omn.}az \sqcap \text{ult.}x^2\,\text{omn.}\frac{az}{x^2} - \text{ult.}x,\ \overline{\overline{\text{omn.omn.}\frac{az}{x^2}}}\,,$$

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[14] I have given this equation, and those that immediately follow it, in facsimile, in order to bring out the necessity that drove Leibniz to simplify the notation.

We have here a very important bit of work. Arguing in the first instance from a single figure, Leibniz gives two general theorems in the form of moment theorems. The first is obvious on completing the rectangle in his diagram, and this is the one to which the given equation applies. In the other the whole, of which the two parts are the complements, is the moment of the completed rectangle; its equivalent is the equation

$$\text{omn.}xy = \text{ult.}x\ \text{omn.}y - \text{omn. omn.}y.$$

Now, although Leibniz does not give this equation, it is evident that he recognized the analogy between this and the one that is given; for he immediately accepts the relation as a general *analytical theorem that he can use without any reference to any figure whatever,* and proceeds to develop it further. This would therefore seem to be the point of departure that led to the Leibnizian calculus.

26 October, 1675.

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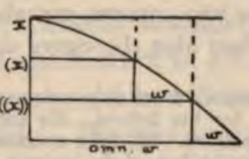

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[14] I have given this equation, and those that immediately follow it, in facsimile, in order to bring out the necessity that drove Leibniz to simplify the notation.

We have here a very important bit of work. Arguing in the first instance from a single figure, Leibniz gives two general theorems in the form of moment theorems. The first is obvious on completing the rectangle in his diagram, and this is the one to which the given equation applies. In the other the whole, of which the two parts are the complements, is the moment of the completed rectangle; its equivalent is the equation

$$\text{omn.}xy = \text{ult.}x\ \text{omn.}y - \text{omn. omn.}y.$$

Now, although Leibniz does not give this equation, it is evident that he recognized the analogy between this and the one that is given; for he immediately accepts the relation as a general *analytical theorem that he can use without any reference to any figure whatever,* and proceeds to develop it further. This would therefore seem to be the point of departure that led to the Leibnizian calculus.

Descartes, Sluse, Gregory St. Vincent, James Gregory and Barrow. Descartes he has already dismissed as impracticable in the manuscript of January, 1675; but there are indications that the former's method has still some influence. An incidental remark leads to the consideration of the *ductus* of Gregory St. Vincent; but these too are soon cast aside, truly because Leibniz does not quite grasp the exact meaning of Gregory. He then either remembers what he has seen in Barrow or refers to it again, for the next thing he gives is some work in connection with which he draws the characteristic triangle, *which is here for the first time, as far as* these manuscripts *go, the Barrow form and not the Pascal form.* He immediately obtains something important, namely,

$$\frac{\overline{\text{omn.}\ l^2}}{2} = \text{omn.}\ \overline{\overline{\text{omn.}\ l}\,\frac{l}{a}}\,.$$

Noting that, in modern notation, l is dy, and a is dx, and also, since a is also supposed to be unity, that the final summation on the right-hand side is performed by "applying the successive values to the axis of x, while the summation denoted by omn.l is a straightforward summation, it follows that the equivalent of the result obtained by Leibniz is $\tfrac{1}{2}y^2 = \int y \frac{dy}{dx} dx$.

However, in attempting to put this theorem into words as a general theorem he makes an error; he quotes $\overline{\text{omn.}l^2}$ as the "sum of the squares" instead of the "square of the final y." This I think is simply a slip on the part of Leibniz, and not, as suggested by Gerhardt and Weissenborn, an indication that Leibniz confused $\overline{\text{omn.}l}^2$ with $\overline{\text{omn.}l^2}$, and considered them as equivalent. Neither of these authorities appears to have noticed the fact that when Leibniz has invented the sign $\int$ (which he immediately proceeds to do) he carefully makes the distinction between the equivalents to the square of a sum and the sum of the squares. Thus we find that his equation is written as

$$\int \frac{\overline{l^2}}{2} = \int \overline{\int l}\, \frac{l}{a}, \qquad \text{(note the vinculum)}$$

while later in the essay we have $\int l^3$ to stand for the sum of the cubes. Further, apart from this. I do not think that any one can impute such confusion of ideas to Leibniz, if it is noted that so far this is not the differential calculus, but the calculus of differences, i. e., l is still a very small but finite line and not an infinitesimal; for in § IV, Leibniz had squared a trinomial successfully, and must have known that the sum of the squares could not be equal to the square of the sum. Both these above-named authorities seem to find some difficulty over the introduction of the letter a, apparently haphazard. This difficulty becomes non-existent, if it is remembered that a is taken to be unity, and the remarks made about dimensions by Leibniz are carefully considered; it will then be found that the a is introduced to keep the equations homogeneous! Weissenborn also remarks that Leibniz jots down the integral of x^2 without giving a proof, and appears to be in doubt how he reached it. If this is so, it confirms the opinion that I have already formed, namely, that neither Gerhardt nor Weissenborn tried to get to the bottom of these manuscripts, being content with simply "skimming the cream."

I suggest that Barrow, Gregory St. Vincent, and even Sluse, now join Descartes on the shelf or the floor, and that the rest of the essay is all Leibniz. He writes the two equations he has found, the equivalents to two theorems obtained geometrically, notes the fact that these are true for infinitely small differences (without, however, mentioning that they are *only* true in such a case), discards diagrams, and proceeds analytically; that is, the y's are successive values of some function of x, where the values of x are in arithmetical progression; hence, substituting x for l in the equation

$$\text{omn.}xl = \text{omn.}l - \text{omn.omn.}l,$$

and remembering that $\text{omn.}x = x^2/2$, as he has proved, we have

$$\text{omn.}\,x^2 = x\,\frac{x^2}{2} - \text{omn.}\frac{x^2}{2}\text{, or omn.}\,x^2 = \frac{x^3}{3}.$$

Again, below he gives $\int \frac{x^3}{3} = \frac{x^4}{4}$ correctly (although there is an obvious slip or, as I think, a misprint of l for x); this could have been obtained in the same way.

$$\text{omn.}\,x^3 = x\frac{x^3}{3} - \text{omn.}\frac{x^3}{3}\text{, or omn.}\,x^3 = \frac{x^4}{4}.$$

Similarly, Leibniz could have gone on indefinitely, and thus obtained the integrals of all the powers of x. But his brain is too active; as Weissenborn says, his soul is in the throes of creation. He merely alludes in passing to the inverse operation to $\int$ as being represented by d, which he for some reason writes in the denominator (probably erroneously because he has noted that $\int$ increases the dimensions); and then he harks back to the opening idea of the essay, the obtaining of the quadratrix by means of transformation of equations, an idea truly as hopeless as the method of Descartes which he has discarded. Nevertheless, even then he obtains something remarkable, nothing more or less than the inverse of the differentiation of a product. This fundamental theorem is obtained geometrically; the proof of the little theorem on which the final result is founded is not given, neither is there a diagram. It cannot therefore be supposed but that Leibniz is working from a diagram already drawn, and I suggest he was referring to one of those theorems, with which he had filled "hundreds of pages" between 1673 and 1675. The proof follows quite easily by the use of the characteristic triangle, and is given in a footnote. This theorem is not in Barrow, nor can I remember seeing it in Cavalieri; I have not yet been able to procure a Gregory St. Vincent; it may be in James Gregory.

The benefits of this discovery are lost as before, for Leibniz once more alludes to the transformation of equations for the purpose of obtaining the quadratrix.

Summing the whole essay, we can say that in it is the beginning of the Leibnizian *analytical calculus*.

29 October, 1675.

Analyseos Tetragonisticae pars secunda.

(Second part of analytical quadrature.)

I think that now at last we can give a method, by which the analytical quadratrix may be found for any analytical figure, whenever that is possible; and, when it can not be done, it will yet always be possible that an analytical figure may be described, which will act as the quadratrix as nearly as is required. This is how I look at it:

Suppose the equation of the curve, of which the quadratrix is required, is given, and that the unknowns in it are x and v. Let the equation to the curve required be[17]

$$v = b + cx + dy + ex^2 + fy^2 + gyx + hy^3 + lx^3 + mxyy + yxx + \text{etc.}; \quad \ldots \quad \text{(i)}$$

let it be set in order for tangents, as follows:

$$-dy - 2fy^2 - gyx - 3hy^3 - 2mxy^2 - mx^2y - \text{etc.}$$
$$= ct + 2ext + gyt + 3lx^2t + my^2t + 2yxt + \text{etc.} \quad \ldots\ldots\ldots\ldots\ldots \text{(ii)}$$

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[17] This is either a misprint, v instead of O, or else Leibniz is in error. For Slusius's method there must be only two variables in the equation. In the *Phil. Trans.* for 1672 (No. 90), Sluse gives his method thus:

If $y^5 + by^4 = 2qqv^3 - yyv^3$, then the equation must be written $y^5 + by^4 + yy^3 = 2qqv^3 - yyv^3$; then multiply each term on the left-hand side by the number of y's in the term, and substitute t in place of one y in each; similarly multiply each term on the left-hand side by the exponent of v; the equation obtained will give the value of t.

The use of the letters v and y is to be noted in connection with Leibniz's use of the same letters; it does not seem at all necessary that Leibniz should have seen Newton's work, with this ready to the former's hand, as a member of the Royal Society. I suggest that Sluse obtained his rule by the use of *a* and *e*, as given in Barrow. Can Barrow's words *usitatum a nobis* (in the midst of a passage written in the first person singular) have meant that the method was common property to himself *and several other mathematicians that were contemporary with him*? This would explain a great deal.

Now, $t/y=a/v$; hence, if from the equation $t/y=a/v$, we eliminate t and y by the help of equations (i) and (ii), that equation should be produced which represents the figure that has to be quadratured; and by comparing the terms of the equation thus obtained with the given equation, unless indeed there is no possibility of comparing them, we shall obtain the quadrature.

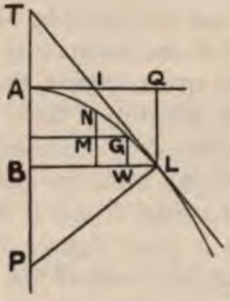
But if an impossibility arises, it is then known that the given analytical figure has no analytical quadratrix. But it is quite clear that if we add to it such as will change it almost imperceptibly, then a quadrible figure may be obtained, since this plainly produces another equation. However, as an impossible case may arise, we must consider the difficulties.

Say that the equation that is obtained is of infinite prolixity, while the given one is finite. My answer is, that in comparing the one with the other it will be seen how far at most the powers of the unknowns in the indefinite equation can go. The retort may be made, that it may happen that the indefinite equation obtained may have more terms than the finite equation that is given and yet may be reduced to it, for it may be divided by something else that is either finite or indefinite. This difficulty hindered me for a long time a year ago, but now I see that we should not be stopped by it. For it may happen that from a certain determinate figure (whose equation is not divisible by a rational) by the method of tangents there may arise an ambiguous figure; for it is impossible to say that, for *any* figure, there shall be only one tangent at any one point. Hence the produced equation can neither be divided by a finite nor by an indefinite quantity; for in truth indefinite figures, or those whose ordinates are represented by an infinite equation, have sometimes these very ordinates finite, and these ought to satisfy the equation. Notwithstanding that, I foresee another difficulty; for indeed it seems that sometimes it may happen that all the roots of the equation will not serve for the solution of the problem. Yet, to tell the truth, I believe they will do so.

Now here is a difficulty that really is great. It may happen that a finite equation may also be expressed as an indefinite one, so that the equation obtained may really be the same as the given equation although it does not appear to be. For example,

$$y^2=x/(1+x)=x-x^2+x^3-x^4+x^5-x^6+\text{etc.};$$

and in the same way others can be formed by various compositions and divisions. This I confess is truly a difficult point, but it can be answered thus: If a figure has an analytical quadratrix of any sort, in all cases it may be assumed to be an indefinite one; and then it will not in all cases give an indefinite, but sometimes a finite, equation that is equivalent to the given equation. In the same way, it is certain that the quadratrix of a given curve as it is usually investigated, whenever there is one, will also be determined; and that too given uniquely and not ambiguously, so that any that differs from it, differs only in name. There is still one difficulty left; it seems impossible to determine which is the end or first term of the indefinite equation that is obtained; for it may happen that the terms of lower degree are cut out, and then it is divisible by y or x or yx or powers of these; nor do I see that there is anything to prevent this. There is the same difficulty whether you start from the lowest or the highest degree in the equation assumed to begin with as indefinite. Suppose then that in the equation obtained this division is possible, then it is necessary that the constant term should be absent, and also all those terms in which x alone or, if you like, all the terms in which y alone is absent; and if we examine this continuously we may light upon an impossibility.



In this general calculus then, we may take it as certain that this difficulty is solved, and that such a division after the calculation can never happen; or if it is possible for it to happen, then the terms will go out, one after the other, so that the equation can be depressed and the comparison be made; and then it is to be seen whether this difficulty cannot be overcome in general, and the comparison proceed as we proceed with the elimination. Perhaps if the figure to be quadratured is reduced beforehand to its simplest equation possible, impossibilities will the more readily be detected. For then presumably the quadratrix must become more simplified. In addition we have another source of assistance; for various calculations leading to the same thing, though obviously differing from one another, can be contrived, from which equations are comparable.

Let $BL = y$, $WL = l$, $BP = p$, $TB = t$, $GW = a$, then $y = \text{omn.}l$.

Incidentally I may remark that there are composite numbers that cannot be added or subtracted from one another by parts, namely those denominated by powers, or by sub-powers or surds. There are also other denominate numbers which cannot be multiplied together by parts, such as numbers representing sums; for instance, omn.l cannot be multiplied by omn.p, nor can we have $y^3 = 2\text{omn. omn.}pl$. However, as such a multiplication may be imagined to occur under certain conditions, we must consider it as follows:

We require the space that represents the product of all the p's into all the l's; we cannot make use of the ductions of Gregory St. Vincent, where figures are multiplied by figures, for by this method one ordinate is not multiplied by all the others, but one into one. You may say that if one ordinate is multiplied by all the rest it will produce a sursolid space, namely, the sum of an infinite number of solids. For this difficulty I have found a remedy that is really admirable. Let every l be represented by an infinitely short straight line WL, that is, we want the quadratrix line representing omn. l; well, the line $BL = \text{omn. } l$; and if this is multiplied by every p, each represented by a plane figure, then a solid is produced. If all the l's are straight lines and all the p's are curves, a curved surface is produced by a duction of the same sort. But these things are all old; now, here is something new.

If upon WL, MG, or every single l, is superimposed the same curve representing all the p's, where the curve p is originally all in the same plane and is carried along the curve AGL while its plane always moves parallel to itself, then what we require will be obtained. In place of a curve a plane may be carried along the curve in the same manner, and a solid will be obtained, whereas by the former method it was a curvilinear surface; and both for the surface and for the solid the section always remains the same. It remains to be seen whether a number of analytical surfaces cannot be ascertained, as in the case of analytical lines; but this is mentioned only incidentally.

N.B. The curvilinear surface formed by the motion of a curve parallel to itself along the curve will be equal to the cylinder of the curve under BL, the sum of all the l's but this is also mentioned incidentally.

To resume, $\frac{l}{a} = \frac{p}{\text{omn.}\, l} = y$, therefore $p = \frac{\overline{\text{omn.}\, l}}{a}\, l$. Hence, omn. $y\, \frac{l}{a}$ does not mean the same thing as omn.y into omn.l, nor yet y into omn.l; for, since $p = \frac{y}{a}\, l$ or $\frac{\overline{\text{omn.}\, l}}{a}\, l$, it means the same thing as omn.l multiplied by that one l that corresponds with a certain p; hence, omn.p = omn.$\frac{\overline{\text{omn.}\, l}}{\text{a}}\, l$. Now I have otherwise proved omn.$p = \frac{y^2}{2}$, i. e., $= \frac{\overline{\text{omn.}\, l^2}}{2}$; therefore we have a theorem that to me seems admirable, and one that will be of great service to this new calculus, namely,

$$\frac{\overline{\text{omn.}\, l^2}}{2} = \text{omn.}\, \overline{\overline{\text{omn.}\, l}\, \frac{l}{a}}, \text{ whatever } l \text{ may be;}$$

that is, if all the l's are multiplied by their last, and so on as often as it can be done, the sum of all these products will be equal to half the sum of the squares, of which the sides are the sum of the l's or all the l's. This is a very fine theorem, and one that is not at all obvious.

Another theorem of the same kind is:

$$\text{omn.}\, xl = x\, \text{omn.}\, l - \text{omn.}\, \text{omn.}\, l\,,$$

where l is taken to be a term of a progression, and x is the number which expresses the position or order of the l corresponding to it; or x is the ordinal number and l is the ordered thing.

N. B. In these calculations a law governing things of the same kind can be noted; for, if omn. is prefixed to a number or ratio, or to something indefinitely small, then a line is produced, also if to a line, then a surface, or if to a surface, then a solid; and so on to infinity for higher dimensions.

It will be useful to write $\int$ for omn., so that

$$\int l = \text{omn.}\, l, \text{ or the sum of the } l\text{'s.}$$

Thus, $$\int \frac{\overline{l^2}}{2} = \int \overline{\int \overline{l}\, \frac{l}{a}}, \text{ and } \int \overline{xl} = x \int \overline{l} - \int \int l.$$

From this it will appear that a law of things of the same kind should always be noted, as it is useful in obviating errors of calculation.

N. B. If $\int l$ is given analytically, then l is also given; therefore if $\int\int l$ is given, so also is l; but if l is given, $\int l$ is not given as well. In all cases $\int x = x^2/2$.

N. B. All these theorems are true for series in which the differences of the terms bear to the terms themselves a ratio that is less than any assignable quantity.

$$\int x2 = \frac{x^3}{3}$$

Now note that if the terms are affected, the sum is also affected in the same way, such being a general rule; for example, $\int\overline{\frac{a}{b}l} = \frac{a}{b} \times \int\overline{l}$, that is to say, if $\frac{a}{b}$ is a constant term, it is to be multiplied by the maximum ordinal; but if it is not a constant term, then it is impossible to deal with it, unless it can be reduced to terms in l, or whenever it can be reduced to a common quantity, such as an ordinal.

N. B. As often as in the tetragonistic equation, only one letter, say l, varies, it can be considered to be a constant term, and $\int l$ will equal x. Also on this fundamental there depends the theorem:

$$\int\overline{\frac{l^2}{2}} = \int\overline{\int\overline{l\,l}}, \text{ that is, } \frac{x^2}{2} = \int x.$$

Hence, in the same way we can immediately solve innumerable things like this; thus, we require to know what e is, where

$$\int\overline{\frac{c}{a}\int\overline{l} + ba^2} + \int l^3 \overset{(18)}{+} \int l^3 = ea^3;$$

we have

$$a^3e = \frac{cx^3}{3} + ba^2x + \frac{x^4}{4} + xa^3.$$

For indeed $\int l^3 = x$, because l is supposed to be equal[19] to a for the purpose of the calculation; $\int\frac{l}{a} = x$.

[18] There is evidently a slip here; l should be x.

[19] This is an instance of the care which Leibniz takes; in the work above l has been the difference for y, and a the difference for x; he is now integrating an algebraical expression, and not considering a figure at all; hence $l = a$, and a is equal to unity, and therefore $\int l^3 = l^3x = a^3x = x$! Thus what is generally considered to be a muddle turns out to be quite correct. The muddle is not with Leibniz, it is with the transcriber. It is certain that these manuscripts want careful republishing from the originals; won't some millionaire pay to have them reproduced photographically in an *edition de luxe*?

Also $\int\overline{c\int\overline{l^2}} = \frac{cx^3}{3}$, that is $= \frac{c\int\overline{l^3}}{3a^3}$, $\int ba^2 = \int l\ ba$. Also it is understood that a is unity. These are sufficiently new and notable, since they will lead to a new calculus.

I propose to return to former considerations.

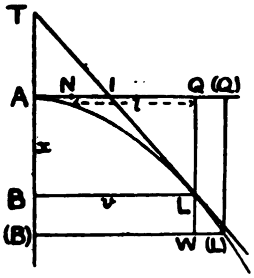
Given l, and its relation to x, to find $\int l$.

This is to be obtained from the contrary calculus, that is to say, suppose that $\int l = ya$. Let $l = ya/d$; then just as $\int$ will increase, so d will diminish the dimensions. But $\int$ means a sum, and d a difference. From the given y, we can always find y/d or l, that is, the difference of the y's. Hence one equation may be transformed into the other; just as from the equation $\int\overline{c\int\overline{l^2}} = \frac{c\int\overline{l^3}}{3a^3}$, we can obtain the equation $c\int\overline{l^2} = \frac{c\int\overline{l^3}}{3a^3d}$.

N. B. $\int\frac{x^3}{b} + \int\frac{x^2a}{e} = \int\overline{\frac{x^3}{b} + \frac{x^2a}{e}}$. And in the same manner, $\frac{x^3}{db} + \frac{x^2a}{de} = \frac{\frac{x^3}{b} + \frac{x^2a}{e}}{d}$.

But to return to what has been done above. We can investigate $\int l$ in two ways; one, by summing y and seeking $\underline{ya/d = 1}$; the other, by summing $z^2/2a = y$, or by summing $\sqrt{2ay} = z$, and then $z^2/t = p = l = ya/d$. Hence, if in an indefinite equation, we eliminate y by substituting in its place $z^2/2a$, and investigate the t of this new equation which is indefinite like the first, and then by the help of the value $z^2/t = l$, and after that by the help of the new value of t, eliminate z from the indefinite equation containing z and t, there will remain out of the (three) letters x, z, t, l, the letter l alone; and again we ought to get an equation which should be the same not only as the given one, but also the same as the one that was obtained a little while ago. Hence, since we have two indefinite equations, containing not only the principle quantities, but also arbitrary ones, yet not altogether unlike the former; and these ought to be identical; it will appear to show whether certain terms cannot be eliminated, whether it is not possible that a comparison should be made, and other things of the sort; and, what is really the most important thing, which terms are really the greatest and the least, or the number of terms of the equation.

Moreover, since in the similar triangles TBL, GWL, LBP, no mention has yet been made of the abscissa x or of the fixed point A, let us then suppose that through the fixed point A there is drawn an unlimited straight line AIQ, parallel to LB, meeting the tangent LT in I; and let AQ = BL; bisect AI in N; then I say that the sum of every QN will always be equal to the triangle ABL, as can easily be shown by what I have said in another place.[20]



These considerations give once more a fresh fundamental theorem for the calculus. For $xv/2=y$, where we suppose that BL $=v$ and QN $=l$, and $y=\int l$;

but $\frac{\text{AI}}{v}=\frac{t-x}{t}$, therefore AI $=\frac{t-x}{t}v$,

and $\text{QI}=v-\text{AI}=v\frac{tt}{t}v+\frac{xv}{2t}$, i. e. $\text{QI}=\frac{xv}{t}$,

$$\therefore \text{QN}=\text{QI}+\frac{\text{AI}}{2}=\frac{xv}{t}+\frac{v}{2}-\frac{xv}{2t}=\frac{xv+tv}{2t}=l.$$ (21)

Now, by the help of the equation $(xv+tv)/2t=l$, and of the former equation $y=xv/2$, and taking once more the first indefinite or general equation as a third, and eliminating first of all y, then t by means of the value found for the ratio of t to x from the indefinite equation containing x and v, and lastly v by the help of the equation $(xv+tv)/2t=l$, in which the principal quantities x and l alone remain, as before; and this again should be identical with the given equation.

Thus we have found three equations obtained in different ways, which should all be identical with one another and with the given equation; and these three are not only identical but should also

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[20] Since the triangles QLI, WL(L) are similar, QI.B(B) = QL.Q(Q), hence omn.QI (applied to AB) = omn. QL (to AQ) = figure AQLA, hence omn.(QI + QA) = rect. ABLQ = 2△ABL.

[21] Since l is the difference for y, therefore $2l$ is the difference for xv; this is shown to be $(xv+tv)/t$ or $x(v/t)+v$; and this is the equivalent to (since $v/t=dv/dx=dv$)

$$d(xv)=xdv+v=xdv+vdx.$$

consist of the same letters and signs; and whether this is possible, will immediately appear on being worked out analytically.

§ VII.

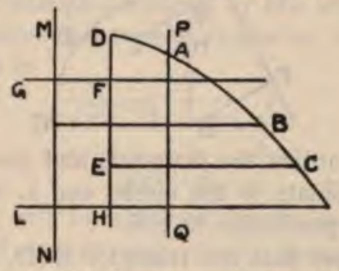
The next manuscript is a further continuation of the preceding, written two days later. In this Leibniz returns to the idea that he has found so prolific, namely, the moments of a figure. It is to be observed that he speaks of the method of breaking up an area into segments as something that he has already worked out; this will be remarked upon in a note on a later manuscript, where it will help to clear up a small difficulty. The accuracy of the rather involved algebraical work is also a point to be noticed.

1 November, 1675.

Analyseos Tetragonisticae pars tertia.

(Third part of Analytical Quadrature.)

It was some time ago that I observed that, being given the moment of a curve ABC, or of a curvilinear figure DABCE, about two straight lines parallel to one another, such as GF, LH (or MN, PQ), then the area of the figure could be obtained; because the two moments differed from one another by the cylinder of the figure, where the altitude was the distance between the parallels.



Now, this is true of every progression, whether of numbers or of lines; that is, even if we do not use curvilinear figures but ordinated polygons; in other words, where the differences between the terms are not infinitely small. Suppose we have any such ordinated quantity z, and let the ordinal number be x, then

$$b\,\overline{\text{omn.}z} \sqcap \pm\,\text{omn.}\overline{zx} \mp \text{omn.}\overline{zx+b}$$

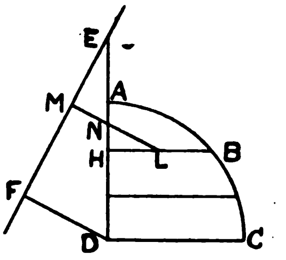
and this is evident by the calculus alone.

By the help of this rule, the sums of terms of an arithmetical progression refolded reciprocally;[22] and this multiplication takes place when it is required to find the moment of the ordinates about a straight line perpendicular to the axis. But if the moment about any other straight line is required, there is the following general rule:

From the center of gravity of each of the quantities of which the moment is required, a perpendicular is drawn to the axis of libration; then the sum of the rectangles contained by the distances or perpendiculars and the quantities will be equal to the moment about the given straight line.

Hence, if the given straight line is the axis of equilibrium, it immediately follows that the moment of the figure about the axis is equal to the sum of the half-squares. Also when it is parallel to that, it will differ from the foregoing by a known quantity.

Now, let us take another straight line: for the circle for instance, let ABCD be a quadrant, vertex B, and center D; let another straight line be given, that is to say, let the prependicular DF be given and



also EF where it meets the diameter, and thus also DE; let HB be the general ordinate to the circle, and L its middle point; let LM be drawn perpendicular to EF.

Then it is clear that the triangles EFD, EMN (where N is the intersection of ML and AD), and LHN are similar.

Let $AD = x$, then $HL = \frac{y}{2} = \frac{\sqrt{a^2 - x^2}}{2}$. But, on account of the similar triangles, $\frac{NH}{HL} = \frac{DF(=d)}{FE(=f)}$; therefore

$$NH = \frac{d}{2f}\sqrt{a^2 - x^2} = \frac{yd}{2f}.$$

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[22] The meaning of this is probably a series such as that considered by Wallis. If a, $a+d$, $a+2d$, etc. is the arithmetical progression, and l, $l-d$, $l-2d$, etc. is the series reversed, then the series refolded reciprocally is al, $(a+d)(l-d)$, $(a+2d)(l-2d)$, etc. It may however mean the sum of the squares of the arithmetical progression. But the point is not very important.

Hence, $\mathrm{EN}=\mathrm{DE}(=e)-\mathrm{HD}(=x)-\mathrm{NH}\left(=\frac{yd}{2f}\right)=e-x-\frac{yd}{2f}$.

Now $\mathrm{NL}=\sqrt{\mathrm{NH}^2+\mathrm{HL}^2}=\sqrt{\frac{d^2}{4f^2}y^2+\frac{y^2}{4}}=\frac{y}{2}\sqrt{\frac{d^2}{f^2}+1}$;

and $\frac{\mathrm{MN}}{\mathrm{EN}}=\frac{\mathrm{NH}}{\mathrm{HL}}$, or $\mathrm{MN}=\frac{\mathrm{NH.EN}}{\mathrm{HL}}$; thus we have

$$\mathrm{MN}=\frac{dy}{2f}\frac{e-x-\frac{dy}{2f}}{\frac{y}{2}\sqrt{\frac{d^2}{f^2}+1}}=\frac{d}{f\sqrt{\frac{d^2}{f^2}+1}}\overline{e-x-\frac{dy}{2f}};$$

and $\mathrm{ML}=\mathrm{MN}+\mathrm{NL}=\frac{d}{f\sqrt{\frac{d^2}{f^2}+1}}\overline{e-x-\frac{dy}{2f}}+\frac{y}{2}\sqrt{\frac{d^2}{f^2}+1}$;

hence, since $e=\sqrt{f^2-d^2}$, we have[23]

$$\mathrm{ML}=\frac{d\,\overline{\sqrt{f^2-d^2}-x-\frac{d}{2f}y}+\frac{d^2+f^2}{2f}y}{\sqrt{d^2+f^2}}=\frac{d\,\overline{\sqrt{f^2-d^2}-x}+\frac{fy}{2}}{\sqrt{d^2+f^2}}$$

and this calculation is general for any curve, so long as x is always taken as the abscissa and y as the ordinate.

Therefore the rectangle contained by ML and HB $(=y)$, or the moment of each ordinate taken with regard to the straight line EF, or *wa*, will be equal to

$$\frac{d\,\overline{\sqrt{f^2-d^2}y-xy}+\frac{f}{2}y^2}{\sqrt{f^2+d^2}}$$

Hence, omn.w will be obtained from the known values of omn.x, omn.xy, and omn.y^2; also, if any three of these four are given, the fourth is also known.

Now, omn.xy will be equal to the moment of the figure about the vertex, omn. y^2 will be equal to the moment of the figure about the axis; hence, given three moments of the figure, that is to say, the moments about two straight lines at right angles and any third, the area is given.

This theorem, however, is less general than the one that was given before, in the first part of this essay, where it does not matter what the angle between the straight lines may be, if only we are given three moments; but it is always understood that they are in the same plane. (Meanwhile, however, this theorem will suffice for the curve of the primary hyperbola; for, if f is infinite, or if FE and ED are parallel, $dy + y^2/2 = wa$, as has already been proved.)

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[23] The accuracy of the algebra is noteworthy in comparison with the inaccuracies that occur later. There is however a slip: $e^2=f^2+d^2$ and not f^2-d^2; this must be a slip and not a misprint, because it persists throughout. It should be noted that the figure given by Gerhardt is careless in that LM is made to pass through A.

It is to be observed that by other calculation the area of a quantity, whose center of gravity lies in a given plane (even though the whole quantity does not), can be found from three given moments about three straight lines in that plane. From this it is to be seen whether the results obtained, when compared with one another, will not produce something new.

If instead of the moment of a figure we require the moment of all the arcs BP, PC, etc., the perpendiculars are to be drawn from the points B, P, C, etc. only, to the straight line; for it will make no difference whether they are drawn from the end or from the middle of BP, for instance, for the difference between two such perpendiculars is infinitely small. Hence, calling the element of the curve z, the moment of the curve about the straight line EF is

$$\frac{d\sqrt{f^2-d^2}z - dxz + fyz}{\sqrt{d^2+f^2}}$$

Most of the theorems of the geometry of indivisibles which are to be found in the works of Cavalieri, Vincent, Wallis, Gregory and Barrow, are immediately evident from the calculus; as, for instance, that the perpendiculars to the axis are equal to the surface or moment of the curve about the axis, for you find that a perpendicular is equal to the rectangle contained by an element of the curve and the ordinate. Therefore I do not set any value on such theorems, or on those about applications of intercepts on the axis (intercepted between the tangents and the ordinates) to the base. Such theorems bring forth nothing new, except maybe they afford formulas for the calculus.

But my theorem about the dimensions of the segments does bring out a new thing, because the space whose dimension is sought is broken up in a different way, that is to say, not only into ordinates but into triangles. Also perhaps the Centrobaric method yields something new. Maybe an easy method can be obtained, by which without diagrams those things which depend on a figure can be derived by calculus. Gregory's theorem, on ductions of two parabolas,[24] one under the other, equal to a cylinder, is immediately evident by calculus; for the ordinate of a circle $y=\sqrt{a^2-x^2}$, that is, the product of $\sqrt{a+x}$ and $\sqrt{a-x}$; and in the same way, $\sqrt{2av-v^2}=y$, which gives $y=\sqrt{v}$ into $\sqrt{2a-v}$; and these come to the same thing.

If the same ordinate y is multiplied by some quantity z, and afterward by the same $z \pm$ some known or constant number b, the difference between the sums produced will be equal to the cylinder of the figure; so that

$$zy_{,,}-zy+by \sqcap by.$$

Although this is evident in general by itself, yet applications of it are not always evident. For instance, let

$$y=\frac{x^2}{ax-b^2}=\frac{x^2}{\sqrt{ax}+b,\ \sqrt{ax}-b};$$

then, multiplying by $\sqrt{ax}+b$, we have $\dfrac{x^2}{\sqrt{ax}-b}$; . . . . (A)

and, multiplying by $\sqrt{ax}-b$, we have $\dfrac{x^2}{\sqrt{ax}+b}$; . . . . (B)

but, since instead of $\dfrac{ax^2}{ax-b^2}$, we can have $x+\dfrac{b^2x}{ax-b^2}$,

which depends on the quadrature of the hyperbola; and thus if one of the two things, (A) or (B), is given, then the other is also known, supposing that the quadrature of the hyperbola is known.

Suppose that at the points C, D, E of a curve situated in any plane there are imposed, perpendicular to the plane, the ordinates of another curve FGH (not necessarily of the same constitution), in such a manner that the middle point of each of these ordinates lies in the plane; then it is evident that LG, MD, NE, multiplied by FL, GM, HN, (that is, the lines imposed at C, D, E of the curve BCDE) or the rectangles FLG, GMD, HNE, or the duction of these two planes into one another, will be equal to the moment of every LC, MD, NE, etc. Hence, if PR is another axis, and the interval between it and QL is the straight line PQ, the moment about PR differs from that about QL by the cylinder whose base is LC, MD, etc., and whose height is PQ.[25]

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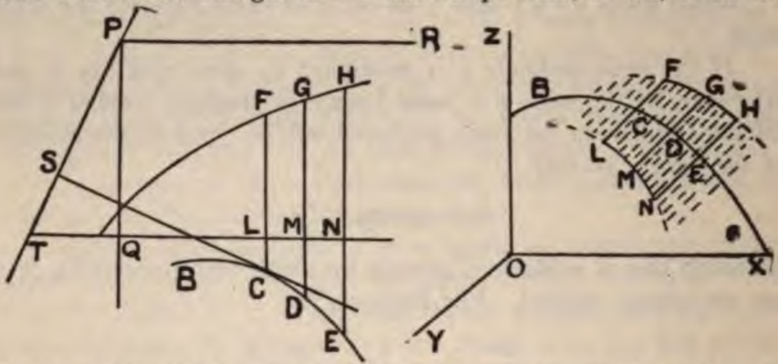
[24] Such theorems are also considered in Wallis, where it is shown that the products for two equal parabolas are the squares on the ordinates of a semicircle; the axes of the parabolas being coincident, but set in opposite sense.

But, if the moment about the straight line PQ, and also that about some other straight line in another position, as TS, of all the ordinates LF of the same figure, imposed at the points C, then we shall have the cylinder corresponding to all the LF's, as I will now prove.



If we call QL, x, and CL, y, then $TC = \frac{f}{a}x + \frac{g}{a}y + h$; and this multiplied by z, where FL or $MG = z$, will give

$$\frac{f}{a}xz + \frac{g}{a}yz + hz.$$

Now xz is given, being the supposed moment about PQ, which is the same whether the z's are placed where they were in the lines LF, MG, etc., or at the points C, D, E. Also yz is given, either as the rectangle FLC or as the duction, by hypothesis. Hence, if in addition there is given one moment of the ordinates imposed upon the curve at the points C, D, E, and this is taken to be equal to $\frac{f}{a}xz + \frac{g}{a}yz + hz$, then we have hz or the cylinder required.

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[25] This is obviously wrong; the base of the cylinder is the area made up of FL, GM, HN, etc. The whole of this last passage proved to be difficult to make out; Leibniz has not completed his figure, by showing the surface formed by placing the ordinates FL, GM, HN with their middle points at C, D, E, and the ordinates themselves perpendicular to the plane of the curve BCDE, which figure I have added on the right-hand side of Leibniz's figure. Even when this is given, there is another difficulty added because as given by Gerhardt, CS is the tangent at D instead of the proper line, namely, the perpendicular from C to TS; in addition through a misprint, this line is afterward referred to as TC. Lastly, "the rectangle FLG" is a misprint for FLC, which with Leibniz stands for FL.LC; this notation for a rectangle is, as far as I can remember, used by Wallis and Cavalieri.

When all these errors are revised, what at first sight seemed to be rather a muddle turns out to be an exceedingly neat idea in connection with the moments of a figure, and their use to find an area, although mostly impracticable.

Note. The values f, g, a, h, are the lengths of TQ, QP, PT, and the perpendicular from Q on PT.

Hence, the curve BCDE is to be chosen such that the ordinates of the given curve can be multiplied by different ordinates of the former, drawn either to the axis QL or to the axis TS, with some advantage of simplicity; and the curves that are suitable for this are those that have several suitable axes, such as the circular or primary hyperbola, which has a pair of asymptotes, or an axis and a conjugate axis.

§ VIII.

Much comment has been made on the fact that the date of the next manuscript was originally "11 November 1675"; that the 5 had been altered to a 3, the ink being of a darker shade; and that it is almost certain that this alteration in date was made for some ulterior motive by Leibniz himself. Hence, if he was capable of falsifying a date in one particular case, then he is not to be trusted in others,. . . ., and so on. Instead of trying to explain away this alteration, let us try to find an explanation as to the reason of its having been made by Leibniz; I offer the following as at least feasible.

The essay starts with the words, "*Jam superiore anno mihi proposueram questionem,. . . .*" I suppose that by this Leibniz intended: "A year or two ago, I set myself the question,. . . . ." This conforms with what follows; the theorem that he sets down is one such as those that were suggested to him by Huygens, and further theorems that came to him as deductions during his first intercourse with Huygens. Years later, I therefore suggest, Leibniz refers to this manuscript, reads his own Latin, *superiore anno,* as "in the above year," gets no further, recognizes the theorem by its figure as one of the Huygens-time batch, and says to himself "1675? No, that's wrong, should be 1673," and proceeds to alter it to what he remembers was the date for the first consideration of the theorem.

N. B. Gerhardt himself has remarked on the darker tint of the ink used in the alteration; hence my argument, made at a later date.

The date 1675 is incontestable; for this composition is quite glaringly a development of the work that has been so efficiently started in that of November 1, 1675. Progress is still delayed by the idea that has obsessed Leibniz up till now, that of the transformation of equations, so as to be able to eliminate more unknowns than the original number of his equations warrant. He sets himself the problem: "To determine the curve in which the distance between the vertex and the foot of the normal is reciprocally proportional to the ordinate," i. e., the solution of the equation $x + y\,dy/dx = a^2/y$, in modern notation. This is a very unlucky choice for him: for I have it on the authority of Prof. A. R. Forsyth that this is incapable of solution in ordinary functions or even by a series in which the law of the series is easily and simply expressible—at least he confesses that he is unable to obtain such a solution, which I take it comes to the same thing.

Leibniz professes to have found the solution and gives $(y^2 + x^2)(a^2 - yx) = 2y^2 \log y$; and unfortunately this false success but enhances the value in his eyes of the method mentioned above. But from the equation given as the solution we may draw an incontestable conclusion; for in a previous problem Leibniz verifies his solution by the method of tangents, i. e., by differentiation, although the method does not as yet convey that idea to him; but he does not verify the solution in this case, *because he is unable at this date to differentiate the product* $y^2 \log y$.

The introduction of dx instead of x/d marks a further advance, more important perhaps than the use of $\int y\,dy$;

for he still writes $\int x$, considering dx to be constant and equal to unity. He is beginning to grasp the infinitesimal nature of his calculus, and that infinitesimals are not to be neglected because of their intrinsic smallness, but because of their smallness *with respect to other quantities* which come into the same equations and are finite; but he is far from being certain about it as yet, as is evidenced by the discussion as to whether $d(v/\psi) = dv/d\psi$ or not. However, the whole manuscript marks a distinct advance on anything that has gone before. From now on he probably discards geometry, and only refers to Descartes, Gregory and Barrow for examples to show how much superior is his method to theirs. I put his final reading of Barrow down to the interval between the date of this manuscript, 11 November, 1675, and November, 1676; it is at this time that he inserts his sign of integration in the margins of the theorems. The next person that examines the originals of these manuscripts (I am convinced that this is very necessary), should carefully see whether the ink used for the note "*novi dudum*" (which I have mentioned) is the same as that used for the sign of integration; also the other books that were used by Leibniz in his self-education should be searchingly scrutinized for clues.

The last remark I have to make is one of astonishment at the errors in the algebraical work which brings this essay to a close, and to a less degree throughout the essay; for we have seen the accuracy to which Leibniz has attained in a previous manuscript; of course, a great deal of erroneous work can be explained by supposing none too careful transcription; but a re-examination of the whole of the Leibnizian remains should include a careful scrutiny on the point as to whether some of the extracts given by Gerhardt are not the work of pupils of Leibniz, whose writing would naturally be somewhat similar. Perhaps too some of those early geometrical theorems might be unearthed; and this would well reward the most painstaking search. Nobody can assert that anything like an adequate tale of the progress of the Leibnizian genius has so far been told.

11 November, 1673.[26]

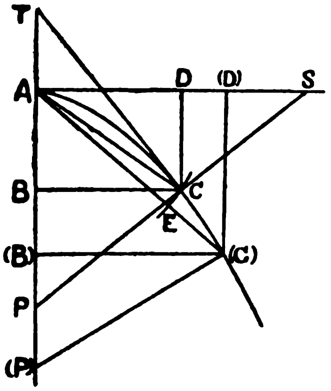
Methodi tangentium inversae exempla.

(Examples of the inverse method of tangents.)

A year or two ago I asked myself the question, what can be considered one of the most difficult things in the whole of geometry, or, in other words, what was there for which the ordinary methods had contributed nothing profitable. To-day I found the answer to it, and I now give the analysis of it.

Find the curve C(C), *in which* BP, *the interval between the ordinate* BC *and* PC *the normal to the curve, taken along the axis* AB(B), *is reciprocally proportional to the ordinate* BC.

Let AD(D) be another straight line perpendicular to the axis AB(B), and let ordinates CD be drawn to it, so that the abscissae AD along the axis AD(D) are equal to the ordinates BC to the axis AB(B), and the ordinates CD to the axis AD(D) are equal to the abscissae AB along the axis AB(B). Let us call AD=BC=y, and AD=BC=x; also let BP=w and B(B)=z. Then it follows from what I have proved in another place that



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[26] See Cantor, III, p. 183; but neither Cantor nor Gerhardt appears to offer any suggestion as to why this date should have been altered.

$$\int wz = \frac{y^2}{2}, \text{ or } wz = \frac{y^2}{2d}$$ [27]

But from the quadrature of a triangle it is evident that $\frac{y^2}{2d} = y$; and therefore $wz = y$.

Now, from the hypothesis, $w = b/y$, for thus w and y will be reciprocally proportional to one another. Hence we have

$$\frac{bz}{y} = y, \text{ and thus } z = \frac{y^2}{b}.$$

But $\int z = x$, *hence* $x = \int \frac{y^2}{b}$; and from the quadrature of the parabola $\int \frac{y^2}{b} = \frac{y^3}{3ba}$; hence, $x = \frac{y^3}{3ba}$; and this is the required equation expressing the relation between the ordinates y and the abscissae x of the curve C(C), which was to be found. Therefore we consider that the curve has been found and it is analytical; in short, it is the cubical parabola whose vertex is A.

We will therefore see whether the truly remarkable theorem is not true, namely, in the cubical parabola C(C), the intervals BP between the normals to the curve, PC, and the ordinates to the axis, BC, taken along the axis ABP, are reciprocally proportional to the ordinates, BC.

The truth of this is easily shown by the calculus of tangents. For the equation to the cubical parabola is $xc^2 = y^3$; taking c to be the *latus rectum*, and supposing that for c^2 we put $3ba$, or $c = \sqrt{3ba}$, we have $3xba = y^3$.

Now, by Slusius's method of tangents, we have $t = y^3/3ba$, where t is put for BT, the interval along the axis between the tangent and the ordinate.

But $BP = w = \frac{y^2}{t}$, and therefore $w = \frac{y^2}{\frac{y^3}{ba}} = \frac{ba}{y}$; hence, the w's and the y's are reciprocally proportional as was to be proved.

[27] This was obtained in the form omn.$p = y^2/2$, previous to October, 1674, from the Pascal form of the characteristic triangle; it is quoted as a known theorem in the essay dated 29 October, 1675. See §§ III, VI.

It is probably at this date that he began to revise his ideas as to d diminishing the dimensions; being forced to reconsider them by the occurrence of such equations as $wz = y$. It is seen in the next paragraph how careful he is to keep his dimensions equal; for he introduces an apparently irrelevant $a(=1)$ for this purpose. It gradually dawns on him that neither $\int$ nor d alter the dimensions, but that a "sum of lines" is really a sum of rectangles, on account of the fact that they are applied in a certain fixed way to an axis; he is not quite certain of this however until well on in the next year, when we find him using $\int dx\, y$.

The artifice of this analysis[28] consisted in obtaining the abscissa from the ordinate; and this idea was never previously thought of. It is not a more difficult question either, if the curve is required in which BP, the interval between the normals and the ordinates, is reciprocally proportional to the abscissae AB. Indeed, $w=a^2/x$; but $w=y^2/2$; hence, we have

$$y=\sqrt{2\int w} \text{ or } \sqrt{2\int\frac{a^2}{x}}.$$

Now $\int w$ cannot be found except by the help of the logarithmic curve.[29] Hence, the figure that is required is that in which the ordinates are in the subduplicate ratio of the logarithms of the abscissae; and this curve is one of the transcendental curves.

Now, in truth, it is a much harder question,[30] if the curve, in which AP is reciprocally proportional to the ordinate BC is required.

For then $$x+w=\frac{a^2}{y} \text{ and } wz=\frac{y^2}{2d}; \text{ also } \int z=x,$$

or $$z=\frac{x}{d}; \text{ thus, } w\frac{x}{d}=\frac{y^2}{2d}, \text{ and } w=\frac{y^2}{2d}\cup\frac{x}{d};$$

hence, $$x+\frac{y^2}{2d}\cup\frac{x}{d}=\frac{a^2}{y}.$$

If we suppose that the x's are in arithmetical progression then $x/d=z$ will be constant, and we shall have

$$x+\frac{y^2}{2d}=\frac{a^2}{y} \text{ or } \int x=\int\frac{a^2}{y}-\frac{y^2}{2},$$

therefore

$$\frac{x^2}{2}+\frac{y^2}{2}=\int\frac{\overline{a^2}}{y} \text{ or } d\,\overline{x^2+y^2}=\frac{2a^2}{y};$$

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[28] It is difficult to see exactly what Leibniz means by this statement; I can only guess at substitution by means of the theorem $wz=y$, the equivalent to the recognition of the fact that $y\,dy/dx.dx=ydy$. The wording is however impersonal, and may mean that he himself had never thought of the idea before.

[29] Required $y=f(x)$, such that $y\,dy/dx=a^2/x$; the solution is $y^2=2a^2 \log_e Ax$. Weissenborn remarks on the omission of the a as being incorrect; from Leibniz's standpoint I cannot agree with him. Leibniz, from Mercator's work, connects a^2/x with the ordinate of the equilateral hyperbola $xy=a^2$, and its integral with the quadrature of this curve. The omission of the a^2 only alters the base of the logarithm, and Leibniz merely states that the solution is of a logarithmic nature without attempting to give it exactly.

[30] How does he know until he has tried it? This rather combats the idea that these were mere exercises; it gives this essay the appearance of being a fair copy intended either for publication or for one of his correspondents. If this were the case, the errors later in algebraical work are all the more unintelligible. The idea that Leibniz was a man who was accustomed to writing down his thoughts as he went along does not appeal to me at all; this is the method of the slow-working mind, rather than that of genius.

but, if we join AC, A(C), then these are equal to $\sqrt{x^2+y^2}$; and if with center A and radius AC we describe an arc CE to cut the straight line AE(C) in E, then E(C) will be the difference between AC and A(C); that is, $E(C)=e=d\overline{x^2+y^2}$

$$\therefore e=2a^2/y.$$

If then it were allowable to assume that the *y's* were also in arithmetical progression, we should have what was required; yet it seems that it does not make any difference even if the *x*'s have been assumed to be in arithmetical progression. For if we do assume that the *x*'s are in arithmetical progression, it follows that the AD's, or the *y*'s are the reciprocals of the E(C)'s or the *e*'s. Moreover, if they are so at any one time they are so at all times. Also, the sums of an infinite number of reciprocal proportionals, no matter what the progression may be of which they are taken as the reciprocal proportionals; for in this case there is not any consideration of rectangles, where there is need of equal altitudes, but a sum of lines is calculated, that of all the E(C)'s.[31] Hence I see the difficulty arise from the fact that the sum of every *e*, or every $2a^2/y$, or every E(C), cannot be obtained, unless we know to what progression the *y*'s belong. In this case, that information is not given; for it is necessary that the *x*'s should be in arithmetical progression, and hence that the *y*'s are not so.

On the other hand, if we suppose in the above equation,

$$x+\frac{y^2}{2d}\smile\frac{x}{d}=\frac{a^2}{y},$$

that the *y*'s are in arithmetical progression, then we have

$$x+\frac{y}{dx}=\frac{a^2}{y}\text{ or }xy+\frac{y^2}{dx}=a^2\,;$$

and, finally, by assigning the progression to neither *x* nor *y*, we have in general

$$xy+y\,\frac{d\dfrac{y^2}{2}}{dx}=a^2\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\text{(A)}$$

But we have not as yet really obtained anything. Let us therefore consider it from the standpoint of "indivisibles"; let PCS produced meet AD in S; then the sum of every AP applied to AB is equal to the sum of every AS applied to AD;[32] or calling DS, v, we have

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[31] This seems to be the root of the error into which he falls; he has not yet perceived that the *e*'s have to be *applied to some axis*, before he can sum them; and this is to a great extent due to the omission of the *dx*, taken as constant and equal to unity. He is thus bound to fall back on the algebraical summation of a series.

$$dy \textstyle\int y + dy \int v = dx \int x + dx \int w,$$
$$\text{or } dy \textstyle\int y + dy \int v = dx \int a^2/y,$$

by the hypothesis of the question.

Now, if we take the y's to be in arithmetical progression, we have

$$\frac{y^2}{2} + \frac{x^2}{2} = dx \text{ Log} y.\ ^{33}$$

But just above, making the same supposition that the y's were in arithmetical progression, we had

$$xy + \frac{y^2}{dx} = a^2 \text{ or } dx = \frac{y^2}{a^2 - xy};$$

and now we have

$$dx = \frac{\overline{x^2 + y^2}}{2 \text{ Log} y}.$$

Hence at length we obtain an equation, in which x and y alone remain, and unshackled, namely

$$\overline{y^2 + x^2},\ a^2 - yx = 2y^2\, \overline{\text{Log}\, y};$$

and this equation, since it is determinate, will give the required locus.

This then is an exceedingly remarkable method, for the reason that when it is not in our power to have as many equations as there are unknowns, yet often we shall be able to obtain some more equations, by the help of which we shall be able to eliminate certain terms, as the term dx in this case, which alone stood in our way. Either of the two equations, by itself, contained the whole nature of the locus, although from neither of them could the solution be derived, because so far easy means were lacking; yet the combination of the two equations gave the solution at once.

I see that the same thing could be otherwise obtained by moments; and here there comes to my mind a new consideration that is not altogether inelegant.

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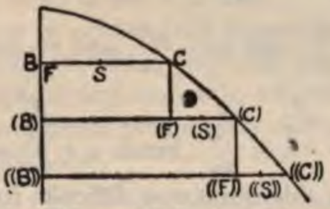
[32] From the characteristic triangle, AS : AP $=$ $dx : dy$.

[33] This is of course nonsense. The error seems to arise from the dx being placed outside the integral sign; thus he assumes that dx is constant, while, for the integration, he also assumes that the dy is constant.

We cannot argue from this equation that Leibniz did not at this date appreciate what an infinitesimal was, on account of the infinitesimal being equated to a finite ratio; for since he is assuming that dy is an infinitely small unit, dx really stands for dx/dy.

In the attached figure, let BC $=y$, FC $=dy$; let S be the middle point of FC; then it is evident that the moment of FC is the



rectangle contained by FC and BS, i. e., the rectangle BFC; this follows from the fact that it is equal to BFC+SFC, and the latter can be neglected as being infinitely small compared to the former.[34]

Hence $\int y\,dy = y^2/2$, or the moment of all the differences FC will be equal to the moment of the last term, and $y\,dy = d(y^2/2)$, or $y^2 dy = y\,dy^2/2$.

Now, just above, in equation (A), by making x arithmetical, we had

$$y\,d\frac{y^2}{2} = a^2 - xy\,,\ \text{or}\ d\frac{y^2}{2} = \frac{a^2 - xy}{y};$$

but this is the same thing as $y\,dy$; hence $y\,dy = \dfrac{a^2 - xy}{y}$, and therefore $\int \overline{y\ \overline{dy}} = \int \overline{\dfrac{a^2}{y}} - \dfrac{x^2}{2}$. But we have already found that $\int \overline{y\ \overline{dy}} = \dfrac{y^2}{2}$; therefore $y^2 + x^2 = 2\int \overline{\dfrac{a^2}{y}}$, as before; i. e., $d\overline{x^2 + y^2} = \dfrac{2a^2}{y}$.

From this there follows something to be noted about these equations, in which occur $\int$ and d, where one quantity, in this case for instance the x, is taken to proceed arithmetically, namely, that we cannot make a change, nor say that the value of x is found, thus, $x = 2(a^2/y) - d\,\overline{y^2}$; for $d\,\overline{y^2}$ cannot be understood unless the nature of the progression of the y's is determinate. But the progression of the y's, in order that it may be used for $d\,\overline{y^2}$, must be such that the x's are in arithmetical progression; hence the dy's depend on the x's, and therefore the x's cannot be found from the dy's. For the rest, by this artifice many excellent theorems with regard to curves that are otherwise intractable will be capable of being investigated, namely, by combining several equations of the same kind.

In order that we may be better trained for really very difficult considerations of this kind, it will be a good thing to attempt just one more, as for instance when the AP's are reciprocally proportional to the AB's.

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[34] Note the advance in ideas suggested by the words "infinitely small *compared with* the former." Here, of course, the notation BFC is the usual notation of the period for BF.FC, the rectangle contained by BF and FC.

Here $x + w = \frac{a^2}{x}$, and $zw = d\frac{y^2}{2}$, and $z = dx$; and so we obtain

$$w = \frac{d\overline{\frac{y^2}{2}}}{z} = \frac{d\overline{\frac{y^2}{2}}}{dx}, \text{ hence } x + \frac{d\overline{\frac{y^2}{2}}}{dx} = \frac{a^2}{x}.$$

The solution of this is not now difficult; for if we suppose that the x's are arithmetical,[35] we have

$$\int x + \frac{y^2}{2} = \int \frac{a^2}{x}, \text{ or } x^2 + y^2 = \overline{\mathrm{Log}\, y}. \qquad (36)$$

Hence, $\sqrt{x^2+y^2} = \mathrm{AC} = \sqrt{2\,\mathrm{Log}\,\mathrm{AD}}$; and this is a simple enough expression for the curve. In this however the AP's are required to be in arithmetical progression; but on the other hand, if the y's are taken to be in arithmetical progression, we have $x + y/dx = a^2/x$; and from this latter the nature of the curve is not easily obtained.

Let us see whether there can be a curve in which AC is always equal to BP; in this case $\sqrt{x^2+y^2} = w$, and $w = dy^2/2dx$. Let the x's be in arithmetical progression then $(\int\sqrt{x^2+y^2}=)\int \mathrm{AC} = y^2$; this, however, is not sufficient to describe the curve practically, that is to say, by points following one another consecutively. When $x=1$, let $\mathrm{BC} = (y)$; then $\sqrt{1+(y^2)} = (y^2)$, or $1 + (y^2) = (y^4)$. Whence (y) may be obtained; thus, from the equation

$$y^4 - y^2 + \tfrac{1}{4} = 1 + \tfrac{1}{4}, \text{ we have } (y^2) = \frac{\sqrt{5}}{2}, \text{ or } (y) = \frac{\sqrt[4]{5}}{\sqrt{2}}. \qquad (37)$$

Further, in the same way,

$$\underset{\mathrm{AC}}{\sqrt{4 + ((y^2))}} + \underset{\mathrm{A(C)}}{\sqrt{1 + \frac{\sqrt{5}}{2}}} = ((y^2));$$

and thus again $((y))$ can be found. By the help of this a third

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[35] Note in general that this is Leibniz's equivalent of the modern phrase, "integrate with respect to x."

[36] This I think is more likely to be a slip on the part of Leibniz, than a misprint; for in the next line he has AD, which is the correct equivalent of y. Further, AP varies inversely as x, hence the AP's have to be in harmonical progression, not arithmetical, otherwise x is not equal to $x^2/2$. If on the other hand, we assume three errors of transcription, and replace x for y, AB for AD, AB for AP, the whole thing is correct with an arbitrary base.

[37] It is hardly necessary to point out the error in the arithmetical solution of the quadratic; nor is it important. It is however to be noted that if $\mathrm{AC} = v$, the equation reduces to $v^2 = x(x+v)$, and the solution is a pair of straight lines.

AC can be found, and some sort of polygon can be found, which is more and more like the curve that is required, in proportion as the thing taken for unity is less and less.

That the x's are in arithmetical progression signifies that the motion (in describing it) along the axis AB is uniform. But descriptions that suppose any motion to be uniform are not within our power.[38] For we cannot produce any uniform motion, except a continually interrupted one.

Let us now examine whether $dx\,dy$ is the same thing as $d\overline{xy}$, and whether dx/dy is the same thing as $d\frac{x}{y}$; it may be seen that if $y=z^2+bz$, and $x=cz+d$; then

$$dy=z^2+2\beta z+\beta^2,\ +bz+b\beta,\ -z^2-bz,$$

and this becomes $dy=\overline{2z+b}\beta$.

In the same way $dx=+c\beta$, and hence

$$dx\,dy=\overline{2z+b}\,c\beta^2.$$

But you get the same thing if you work out $d\overline{xy}$ in a straightforward manner. For in each of the several factors there is a separate destruction, the one not influencing the other; and it is the same thing in the case of divisors.

Now let us see if there is any distinction when we seek the sums of these things. We have $\int dx=x$, $\int dy=y$, and $\int d\overline{xy}=xy$. If then we have an equation, $dx\,dy=x$ say, then $\int dx\,dy=\int x$. But $\int x=x^2/2$, hence $xy=x^2/2$, or $x/2=y$; and this satisfies the equation $dx\,dy=x$; for substituting for y its value, $ax\frac{dx}{2}=x$, or $a\frac{x^2}{2}=x$, (39) which is known to be true.

In sums these results do not hold good; for $\int x\int y$ is not the same thing as $\int xy$; the reason is that a difference is a single quantity, while a sum is the aggregation of many quantities. The sum of the differences is the latest term obtained. However, from the sums of the factors we can find the sums of products, not indeed as yet analytically, but by a certain method of reasoning; such as Wallis has done in this class of thing, not by proving them, but by a happy method of induction. Nevertheless to find proofs for them would be a matter of great importance.

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[38] This is strongly reminiscent of Barrow, Lect. I (near the beginning) and Lect. III (near the end).

[39] Leibniz, as a logician, should have known better than to trust a single example as a verification of an affirmative rule.

With regard to infinitesimals note the equation $dx\,dy=x$!

Suppose $\int \overline{zy}$ to be the sum that is required. Let $\int \overline{zy} = w$, then $zy = \overline{dw}$, and $y = \frac{\overline{dw}}{z}$, and $\int y = \int \overline{\frac{dw}{z}}$. Similarly, $\int z = \int \overline{\frac{dw}{y}}$. Suppose that $\int y$ is known, $= v$, and that $\int z$ is known, $= \psi$; then $y = dv = \frac{dw}{z}$, and $z = d\psi = \frac{dw}{y}$, and $\frac{dv}{d\psi} = \frac{z}{y}$. From this it would seem to follow that $d\frac{v}{\psi} = \frac{z}{y}$, and therefore that $\frac{v}{\psi} = \int \frac{z}{y}$. Therefore $\int \frac{z}{y} = \frac{\int z}{\int y}$, which is obviously incorrect. (40) Hence it follows that $\int \frac{dv}{d\psi}$ cannot be equal to $\frac{v}{\psi}$.

What then can it be? We have to sum the difference for v divided by the difference for ψ. That is, not every one of the differences for, or the whole of, v is to be divided by each single difference for the ψ; this is not so, I say, because each single one of the first set is only divided by the single one of the other set that corresponds to it, and not by all of them. Therefore $\int \frac{dv}{d\psi}$ is not the same as $\frac{\int dv}{\int d\psi}$, or $\frac{v}{\psi}$. Will not then $d\frac{v}{\psi}$ be something different from $\frac{dv}{d\psi}$? If it is the same, then also $\int d\frac{v}{\psi} = \int \frac{dv}{d\psi}$, that is $\frac{v}{\psi} = \int \frac{dv}{d\psi} = \frac{\int dv}{\int d\psi}$, which is absurd.

Similarly, if we can suppose that $d\overline{v\psi} = dv\, d\psi$, then $\int \overline{dv\psi}$, or $v\psi = \int \overline{dv\, d\psi}$. Now $v\psi = \int dv \int d\psi$; hence, $\int \overline{dv\, d\psi} = \int dv \int d\psi$; which is absurd.

Hence it appears that it is incorrect to say that $dv\, d\psi$ is the same thing as $dv\psi$, or that $\frac{dv}{d\psi} = d\frac{v}{\psi}$; although just above I stated that this was the case, and it appeared to be proved. This is a difficult point. But now I see how this is to be settled.

If we have v and ψ, and they form some quantity, say $\phi = v\psi$ or v/ψ, and if the values of v and ψ are expressed as rationals in terms of some one thing, for instance, in terms of the abscissa x, then the calculus will always show that the same difference is produced, and that $d\phi$ is the same as $dv\, d\psi$ or $dv/d\psi$. But now I see

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[40] If Leibniz can see that this equality is "obviously incorrect," what is the use of the argument that has preceded this sentence; for the final result must also be obviously incorrect.

the former can never happen, nor can it come to the latter by separation of parts; for example,

$$x+\beta, \frown x+\beta,, -, x, x, \text{ becomes } 2\beta x,$$

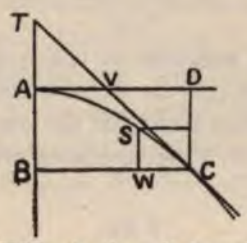
which is quite a different thing from

$$x+\beta, -x,, \frown x+\beta, -x \text{ which gives } \beta^2.$$

Hence it must be concluded that $dv\psi$ is not the same as $dv\,d\psi$, and

$$d\frac{v}{\psi} \text{ is not the same as } \frac{dv}{d\psi}. \quad (41)$$

Take an equation of the first degree, $a+bx+cy=0$. Let DV $=\theta$, AB $=x$, BC $=y$, and TB $=t$. Then, by making use of the method of tangents,[42] we have $bt=-cy$, or $t=-cy/b$. In the same way, $\theta=-bx/c$.



Let WC $=w$, and WS $=\beta$, then it is evident that $t/y=\beta/w$, and therefore $w=-\beta\dfrac{b}{c}$, and in the same way, $\beta=\dfrac{-wc}{b}$.

Second degree. $a+bx+cy+dx^2+ey^2+fyx=0$. Making use of the method of tangents, we have

$$bt+2dxt+fyt=-cy-2ey^2-fyx;$$

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[41] Leibniz here justifiably verifies the falsity of his supposition being a general rule by a single breach of it. He uses $v=\psi=x$, and changes x into $x+\beta$; thus,

$$\begin{aligned} d(xx) &= (x+\beta)(x+\beta)-xx &&= 2\beta x \\ dx\ dx &= (x+\beta-x)(x+\beta-x) &&= \beta^2. \end{aligned}$$

Here we see the first idea of the method that is the same as that used by Fermat and, afterward by Newton and Barrow; this consideration, whatever the source, is that which leads him later to the substitution $x+dx$, $y+dy$ in those cases in which Barrow uses a and e.

[42] "ordinando et accommodando," literally setting in order and adapting. It is to be remembered that Sluse gave only a rule, and not a demonstration of the rule. Part of the rule was that, if the equation in two variables contained terms containing both the variables, these terms had to be set down *on each side of the equation.* Thus, for the equation $y^3=bvv-yvv$ would first of all be written

$$y^3+yvv=bvv-yvv \text{ .... ordinando (?)}$$

then each term on the left is multiplied by the exponent of y, and each term on the right by that of v, thus,

$$3y^3+yvv=2bvv-2yvv \text{ .... accommodando (?)}$$

and finally one y on the left, in each term, is changed into a t, where t is the subtangent measured along the y axis.

hence $t=\frac{-cy-2ey^2-fyx}{b+2dx+fy}$. From this it is quite evident that t can always be divided by y (and θ by x), and since $w=\beta y/t$, therefore we have

$$w=\frac{\beta\overline{b+2dx+fy}}{-c-2ey-fx},\ \text{and}\ y=\frac{-w\,\overline{c+fx},\frown\beta\overline{b+2\,dx}}{f+2e},$$

but from just above $y=\frac{-a-bx-dx^2}{c+ey+fx}$, hence we have

$$y=\frac{-w,\overline{c+fx},\frown\beta\overline{b+2\,dx},\frown\overline{c+fx},,,+\overline{f+2e}\frown\overline{a+bx+dx^2}}{-w,\overline{c+fx},\frown-\beta\overline{b+2dx},\frown-e}\qquad\text{(43)}$$

$$=\frac{-w\,\overline{c+fx},-\beta\overline{b+2dx}}{f+2e}.$$

Hence. we have an equation in which there is no longer any y;[44] and all figures that can be formed from this equation by a variation of the letters that stand for the constants can be squared; and also all others that by other methods can be shown to be connected with it.

§ IX.

In the manuscript that follows we must refrain from being critical; for, as suggested by the opening remark, it contains nothing more than random notes, jotted down as they came into Leibniz's mind, as materials for further investigation. In the ten days that have intervened since the date of the last MS., he has either had no spare time for further work on the lines of this last manuscript, or else he has found that he cannot proceed any further use-

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[43] This is hopelessly inaccurate; all except one error, namely, $f+2e$, which should be $\beta f+2ew$, may be put down to bad transcription. Even if Leibniz's writing were execrable, the correct version of an ambiguous sign (through bad writing) could easily have been settled, *by working through the algebra.* Thus the first of the last pair of values, in Leibnizian symbols should be

$$y=\frac{-w,\overline{c+fx},-\beta,\overline{b+2dx},,\frown\overline{c+fx},,,+\overline{\beta f+2ew},\overline{a+bx+dx^2}}{-w,\ c+fx,\ -\beta,\ b+2dx,,\ \smile-e,}$$

with a similar correction in the second value.

[44] Even if Leibniz had worked out the correct result, and obtained what he was trying for, namely, w/β in terms of x, he would have got a very lengthy quadratic, and the roots would be quite beyond his power to use at any time. But he convinces himself that he can thus find the quadrature of any conic, or figures that can be reduces to them.

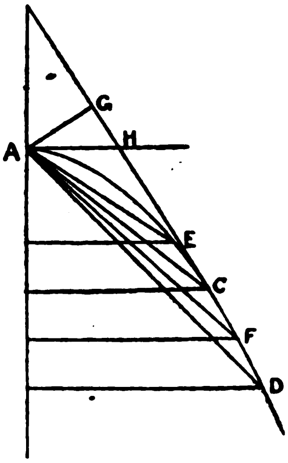
fully until he has perfected the method he had in hand. He therefore reverts to the method of breaking up the figure into triangles by means of a set of lines meeting in a point, coupled with the ideas of the moment and the center of gravity, in order to try to obtain further general theorems for *analytical use*. In this way, he again comes across the differentiation of a product in the form of an "integration by parts"; but he does not recognize in it the differentiation of a product, for he says that as he has obtained this before he can get nothing new from it. He is still wasting his energies over the idea of obtaining dy/dx as an explicit function of x, for the purposes of *integration* or quadratures. The fact that he can use the method of Slusius *as an unproved rule* seems to have hidden from him the necessity of pushing on his investigations with regard to the laws of *differentiation,* or the direct tangent method.

21 November 1675.

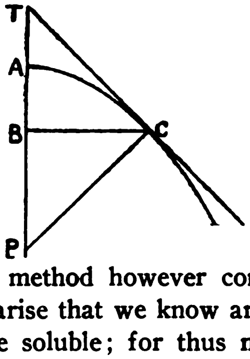
> *Pro methodo tangentium inversa et aliis tetragonisticis specimina et inventa. Trigonometria indivisibilium. Aequationes inadaequatae. ordinatae convergentes. Usus singularis Centri gravitatis.*
>
> [Examples and discoveries by means of the inverse method of tangents and other quadratures. Trigonometry of indivisibles. Inadequate equations. Converging ordinates. Special use of the Center of Gravity.]

Subject-matter for a new consideration of the Center of Gravity method, as follows:

A segment AECD having been broken up into infinite triangles, AEC, ACF, etc., let the center of gravity of each of these triangles be found; this is a simple matter, for the center of gravity is always distant from the base a third of the altitude. Then, since the path of the center of gravity multiplied by the area of the triangle is equal to the solid formed by its rotation, and also since the products of the AH's and the infinitesimal parts of the axis are twice the areas of the triangle, also it is plain that the AG's multiplied by the distances of the centers of gravity of the triangles AEC from the axis are equal to the moment of the segment about the axis; by the help of this idea a number of things can be at once obtained in two ways: first, by taking some general figure and making a general calculation, and then so expressing it that the center of gravity can be easily found; in this way we may obtain the moments of spaces which would be a matter of difficulty otherwise, if they were investigated by the ordinary method of ordinates.



Secondly, on the other hand, if figures of which the moments are easily obtained in the ordinary way are treated by this method, we shall arrive at certain very difficult curves, the dimensions of which can always be deduced from some that are easier. Here then we have a remarkable rule, by the help of which useful properties can always be obtained from any method however complicated. It is often useful when problems arise that we know are naturally simple, and from other reasons are soluble; for thus many notable cases are discovered. See what Tschirnhaus noted about the Hastarian line.



In irregular problems, such as cannot be treated in a straightforward manner or reduced to an equation that is sufficiently determinate, because, say, something has to be done inversely, it is useful to compare several ways with one another, of which the results should be identical. This seems to be useful for the inverse tangent method. Here is a case in point.

The figure, in which BP and AT are reciprocally proportional, is required.

Let $TB=t$, then $AT=t-x$, and $BP=a^2/(t-x)$. If this is multiplied by t, we have

$$\square TBP=ta^2/(t-x)=a^2+a^2x/(t-x)=y^2$$

hence. $$ta^2=ty^2-xy^2,$$

or $t=xy^2/(a^2-y^2)$;[45] and therefore $t/x=y^2/(a^2-y^2)$, or all the t's together equal the moment about the vertex of every $y^2/(a^2-y^2)$.

But from other reasons, all the TP's applied to the axis are equal to the TC's applied to the curve.

Now $t/y=\beta/w$, and therefore $w=\dfrac{\beta y}{t=\dfrac{y^2x}{a^2-y^2}}=\dfrac{\beta\overline{a^2-y^2}}{xy}$.

But $\int w=y$, therefore

$$\int\overline{\frac{\beta\overline{a^2-y^2}}{xy}}=y\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(A)$$

Further, $$wx=\frac{\beta\overline{a^2-y^2}}{y},\text{ and }\int\overline{wx}=yx-\int y\beta,$$

hence, $$\int\frac{\beta\overline{a^2-y^2}}{y}=yx-\int y\beta\ldots\ldots\ldots\ldots\ldots\ldots(B)$$

Also $$w=dy,\ dy=\frac{\beta\overline{a^2-y^2}}{xy},\text{ and therefore}$$

$$xy=\frac{\beta\overline{a^2-y^2}}{dy=w}=\int\overline{}y\beta+\int\overline{\frac{\beta\overline{a^2-y^2}}{y}}.$$

Now if we suppose that the y's are in arithmetical progression, then $w=dy$ is constant and β is variable;

hence, $$\beta=\frac{\int\overline{y\beta+\beta\frac{a^2-y^2}{y}}}{a^2-y^2},\ \overline{d\,\beta\overline{a^2-y^2}}=\frac{a^2\beta}{y}.$$

But from equation (B), $\beta\dfrac{a^2-y^2}{y}+\beta y=\overline{dyx}$

hence, $$\beta\frac{a^2}{y}=\overline{dyx}.$$

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[45] There is a mistake in sign; a^2-y^2 should be y^2-a^2; hence the work that follows is also wrong.

We have thus obtained two equations that are mutually independent, the first $\frac{dx}{dy} = \frac{yx}{a+y,\, a-y}$ (46) ....(1)

and the second $\overline{dyx} = \frac{dx\, a^2}{y}$. ....(2)

Let us seek to obtain others in addition, such as

$$\int t\, dy = \int y\, dx.$$

Now this furnishes us with nothing new; but $\int tw + \int xw = xy$ or $t\,dy + x\,dy = \overline{dxy}$, and $t = \frac{dx}{dy}\,y$; hence the latter $= \frac{\overline{dxy} - x\, dy}{dy}$. Therefore $\overline{dx}\, y = \overline{dxy} - x\, \overline{dy}$.

Now this is a really noteworthy theorem and a general one for all curves. But nothing new can be deduced from it, because we had already obtained it.

However, from another principle we shall obtain a new theorem; for it is known that the sum of every $BP = BC^2/2$; that is to say, $BP = \frac{a^2}{t-x}$, $t = \frac{\beta y}{w} = \frac{\overline{dx}}{dy}\,y$, and therefore

$$BP = \frac{a^2 dy}{\overline{dx}\, y - \overline{dy}\, x} = \frac{\overline{dy^2}}{2} \quad \ldots\ldots\ldots\ldots\ldots (3)$$

We therefore have two equations, in which dx occurs, namely, the first and the third; by the help of these, by eliminating dx, we shall have an equation in which only one of the unknowns remains shackled; thus from equation (1), we have $dx = \frac{\overline{dy}\; yx}{a^2 - y^2}$, and now from equation (3), we get $\overline{dx}\, y\, \overline{dy^2} - dy\, \overline{dy^2} x = 2a^2\, dy$. Hence,

$$dx = \frac{2a^2 \overline{dy} + dy\, \overline{dy^2} x}{y\, \overline{dy^2}}.$$

We have therefore an equation between the two values of dx, in which only the y remains shackled. From this, by assuming

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[46] Although the variables are separable, Leibniz does not recognize the fact that he can make use of this. For later he states that the solution of a problem cannot be obtained from a single equation. In this case we have

$$\frac{dx}{x} = \frac{y\ dy}{y^2 - a^2} = \frac{dv}{v}, \text{ if } y^2 - a^2 = \pm v^2.$$

Supposing this substitution to have been effected, Leibniz would have concluded that $x = v$, and would have stated that he had solved the problem.

But here again he has made an unfortunate choice, for the origin (A) cannot fall on any of the curves $Cx = v$ or $Cx^2 \pm y^2 = \pm a^2$, which is the general solution of the equation. Hence the problem is impossible.

the *y*'s to be in arithmetical progression, that is that $dy=\beta$ a constant, and $\overline{dy^2}=z$, and $z=z^2/2=y^2$; $z=\sqrt{2}\,y=\overline{dy^2}$.[47] Thus we have obtained what was required.

We have here a most elegant example of the way in which problems on the inverse method of tangents are solved, or rather are reduced to quadratures. That is to say that the result is obtained by combining, if possible, several different equations, so as to leave one only of the unknowns in the tetragonistic shackle. This can be done by summing ordinates in various ways, or on the other hand, instead of ordinates, converging or other lines.

Note. If, instead of x or y, some other straight line can be found, either one that is oblique, or one of a number converging to the same point, by the employment of which one only of the unknowns is left in bonds, it may be employed with safety. Take for instance the case of finding the relation for the AP's; here the sum of AP's applied to the axis is half the square on AC. Whenever the formula for the one unknown that is left in shackles is such that the unknown is not contained in an irrational form or as a denominator,[48] the problems can always be solved completely; for it may be reduced to a quadrature, which we are able to work out; the same thing happens in the case of simple irrationals or denominators. But in complex cases, it may happen that we obtain a quadrature that we are unable to do. Yet, whatever it may come to, when we have reduced the problem to a quadrature, it is always possible to describe the curve by a geometrical motion; and this is perfectly within our power, and does not depend on the curve in question. Further, this method will exhibit the mutual dependence of quadratures upon one another, and will smooth the way to the method of solving quadratures. Meanwhile I confess that it may happen that there may be need for a very great number of inadequate equations (for so I call them, when there is need for many to solve the problem, although each alone would suffice provided it could be worked out by itself), in order to completely free one of the unknowns from its shackles. For, unfortunately, a solution cannot be obtained from a single equation, unless one of the terms is free from shackles; and if this term appears oftener, then not unless it is freed at least once. Thus there may be a great number of inadequate equations to be found; and we have to examine which of them are in some way independent of the others, i. e., such as cannot be derived from one another by a simple manipulation; for instance, the sum of all the AP's and the sum of all the AE's.

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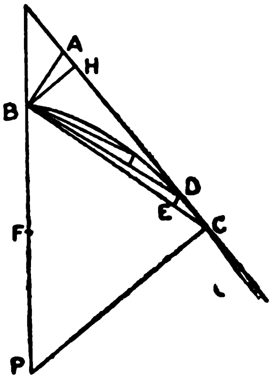
[47] This is quite unintelligible to me as it stands; query, is it an accurate transcription?

[48] This is tantamount to a confession by Leibniz that he cannot explicitly integrate $\int a^2/y$, although he knows that it is logarithmic or reduces to the area under the hyperbola; for he has given this in the MS. for Nov. 11.

A new kind of Trigonometry of indivisibles, by the help of ordinates that are not parallel but converge.

Let B be a fixed point; let BDC be a very narrow triangle standing upon a curve; let DE be the perpendicular to BC; from the point B let BA, perpendicular to BC or parallel to DE, be drawn to meet the tangent AHDC, and let BH be the perpendicular to the tangent DC produced.



Then the triangles CED, CHB, BHA are similar; hence we have $BH/CE = HA/DE = BA/CD$, and therefore $BH, DE = CE, HA$, and $BH, CD = CE, BH$. Hence it follows that the sum of the triangles or the area of the figure is equal to the products of the AB's into the CE's, or the differences of the ED's and lastly $AH, CD = DE, BH$.[49]

Further, $CH/CE = HB/DE = CB/CD$; hence, again, $CH, DE = CE, HB$, and $HB, CD = DE, CB$; i. e., the area of the triangle, as is in itself evident, is equal to itself. Lastly, $CH, CD = CE, CB$; and this last result seems to be worth noting for the case of a Trochoid.

For, if by the rolling of a curve DC on a fixed plane CA, a trochoid curve is described by the point B fixed in DC, and it is given that the ordinate of the trochoid drawn to the fixed plane CA is BH, then the sum of the intercepts CH applied to DC will be equal to the sum of the CB's applied to their own differences. Now if any ordinates are applied to their own differences, the same thing is always produced as in the case where we try to find the moment of the differences about the axis, which is the same as the moment when we take the sum of each, or the maximum ordinate, into the

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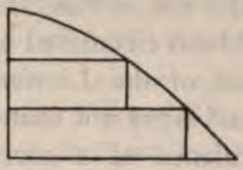
[49] There are several errors in the letters in this paragraph, which are probably due to transcription; thus, an E for a (? badly written) B, an H for an A, etc., would be quite an easily-imagined error, provided the work was not verified during transcription.



distance of its center of gravity from the axis, i. e., its middle point, that is to say into half itself. Finally this is equal to half the square on the maximum ordinate. Therefore we can always obtain the sum of all the rectangles BC, CE, which is always equal to half the square on BC, or to the sum of all the BP's applied to the axis in F, where CP is the normal to the curve DC.

§ X.

Leibniz now directs his attention to the direct method of tangents, and proceeds to generalize the methods of Descartes. Is it only a coincidence that Barrow uses this method regularly, the curve that he is especially partial to being the rectangular hyperbola? Weissenborn suggests the same coincidence occurs with respect to the method of Newton, who uses analytical approximations; but if there is anything in either of these suggestions. I think that the Barrovian idea, which is purely for the construction of tangents, is much nearer to that of Leibniz in this manuscript than is the Newtonian.

However this may be, Leibniz is at last beginning to consider the point as to the method by which the principle of Sluse is obtained. He ascribes it to a development of the method of Descartes; but in this connection I cannot get out of my head the suggestion raised by Barrow's use of the first person *plural*, "frequently used by *us*," in the midst of a passage that is written, contrary to his usual custom, in the first person *singular* throughout, where he describes the differential triangle and the "a and e" method. I consider that Sluse has enunciated a *working rule* for tangents, which he has generalized by observation of the results obtained by the use of the "a and e" method; and that this method had been circulated by Barrow some time before the publication of the *Lectiones Geometricae,* although I confess that I have not found any record of this, nor any distinct evidence of a correspondence between Barrow and Sluse; but there is more than a suggestion of this in the fact that Sluse's article was published in the *Phil. Trans.* for 1672.

It seems more than strange to me that there should be such a prolific crop of differential calculus methods within a couple of years of the work of Barrow in all sorts of places, raised by many different people, and that none of them allude to the general seed-merchant, as I consider Barrow to have been.

22 Nov. 1675.

Methodi tangentium directae compendium calculi, dum jam inventis aliarum curvarum tangentibus utimur. Quaedam et de inversa methodo.

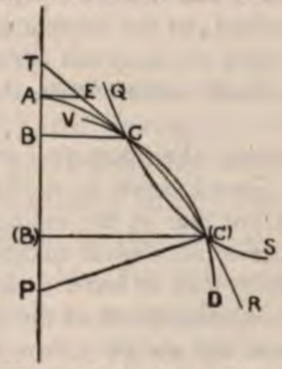
[Compendium of the calculus of the direct method of tangents, together with its use for finding tangents to other curves. Also some observations on the inverse method.]

In that which I wrote on Nov. 21, I noted down those things which came to my mind concerning the method of tangents. Returning to the subject, let ACCR and QCCS be two curves that cut one another in one, two, or more points C, C; let AB(B) be the axis; let $AB=x$ be the ordinates, and $BC=y$ the abscissae; then we shall have two equations to the two lines, each in terms of these two principal unknowns. Now if these two equations have equal roots, or the equations have equal values, then the lines will touch one another. Instead of the line QCCS, Descartes chooses the arc of a circle VCCD, whose center is P, so that PC is the least of all the lines that can be drawn from the point P. It will come to the same thing, and often more simply, if we take not the arc of a circle but the tangent line TC(C), that is the greatest of all those that can be drawn from a given point T to the curve. Let TA$=b$, AE$=e$, be assumed as given; required to find AB, BC. The two equations are, the one for the curve AC(C), namely, ax^2+cy^2+etc. $=0$, and the other to the straight line TC(C) which, on account of the relation TA/AE$=$TB/BC, will be $b/e=(b\pm x)/y$ or $\pm x=(b/e)y-b$

or $y=\pm (e/b)x+e$.

Thus the value of either one or other of the unknowns can always be obtained explicitly, and thus can be worked out immediately without raising the degree of the equation of the given curve AC(C); and then at once we shall obtain an equation giving the unknown that alone remains, so that we may determine the condition for equal roots. Doubtless this is the principle of Sluse's method.

If however the arc of the circle whose center is P is used, following Descartes, then the new equation, for the circle, will be as follows: let the radius PC$=s$, and PB$=v-$x, and we have



$s^2=y^2+v^2+x^2-2vx$. Hence it is clear that we have the choice of either a circle or a straight line; and when, in the equation to the given curve, only an even power of y appears (as can always be made to happen in the case of the conics), then it will be more convenient to use equations to circles; for thus, by the help of the two values of y^2, the unknown x can be immediately worked out; but, in general for all equations to curves expressed by a rational relation, the method of the straight line may be usefully employed.

Hence I go on to say that not only can a straight line or a circle, but any curve you please, chosen at random, be taken, so long as the method for drawing tangents to the assumed curve is known; for thus, by the help of it, the equations for the tangents to the given curve can be found. The employment of this method will yield elegant geometrical results that are remarkable for the manner in which long calculation is either avoided or shortened, and also the demonstrations and constructions. For in this way we proceed from easy curves to more difficult cases, and an equation to a curve being supposed known, it is always possible to choose an equation to some other curve whose tangents are known, by the help of which one of the unknowns can be worked out very easily.

Thus, if it is given that $hy^2+y^3=cx^3+dx^2+ex+f$ is the equation to a curve of which the tangents are required, assume a curve of which the equation is $hy^2+y^3=gx+q$, for that of which the tangents are already known; eliminating y, we have an equation such as $gx+q=cx^3+dx^2+ex+f$. This can be determined for two equal roots, either by Descartes's method of comparisons, or Hudde's by means of an arithmetical progression; and thus by working out the value of x, the value of either g or q may be found; and one of the two letters q or g can be chosen arbitrarily.[50] Hence, a way of describing that other curve that touches the given curve is obtained; now, when this is described, let the tangent be drawn at the point which is common to it and the proposed curve, which tangents we have supposed to be already known; then this tangent will touch the given curve.

I think that, in general, the calculation will be possible by this method of assuming a second curve, as we have done in this case, which evidently works out one of the unknowns. Hence I fully believe that we shall derive an elegant calculus for a new rule of tangents, which in addition may be better than that of Sluse, in that it evidently works out immediately one of the two unknowns, a thing that the method of Sluse did not do. Now this very general and extensive power of assuming any curve at will makes it possible, I am almost sure, to reduce any problem to the inverse method of tangents or to quadratures. Indeed let any property of the tangents to a curve be given, and let the relation between the ordinates and the abscissae be required. Then an equation can be derived, which will contain the principal unknowns, x, y, and always two others as incidentals, such as s and v, or b and e, or the like; now, as the equation contains the property of the tangents, by which s and b may be expressed so as to have a relation to the tangents, assume in this case any new curve chosen arbitrarily, and then s and v will also have some known relation to this curve. By means of the equation to the arbitrarily chosen curve, we shall be able to replace the given property of tangents in favor of the curve required, namely, by removing one or other of the unknowns; and by thus reducing the problem to such a state the inverse calculation will come out the more easily.

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[50] The method of Hudde appears to be similar in principle to that of Sluse, while that of Descartes was the construction of the derived function by assuming roots, forming the sum of the quotients of the function divided by each of the assumed root-factors in turn, and comparison with the original function. Both therefore reduce to finding the common measure of the equation to the curve (where the right-hand side is zero) and the differential of it.

Leibniz, however, strange to say, does not note that by taking one of his arbitrary constants, q, equal to f, the equation has its degree lowered in the particular case he has chosen.

The whole thing, then, comes to this; that, being given the property of the tangents of any figure, we examine the relations which these tangents have to some other figure that is assumed as given, and thus the ordinates or the tangents to it are known. The method will also serve for quadratures of figures, deducing them one from another; but there is need of an example to make things of this sort more evident; for indeed it is a matter of most subtle intricacy.

The manuscripts mentioned above seem to be all that were found by Gerhardt belonging to the period 1673-5. I feel that it is a great pity that they were not given in full, or at least a little more fully. For instance, Gerhardt mentions that Leibniz in the MS. of August 1673 constructs the so-called characteristic triangle, but does not give Leibniz's figure in connection. This figure should have been given; for the figure given in October 1674 is not the characteristic triangle as given by Leibniz in the "postscript" (§ I), or the *Historia* (§II), but it *is the Pascal diagram* (assuming that the figure given by Cantor is the correct one). It would be useful to know the date at which Leibniz drops the Pascal diagram in favor of one or other of the Barrow diagrams.

It is to be noticed at this date that Leibniz uses one infinitesimal only, and *verifies* that the method of Descartes comes out correctly in the simple case of the parabola; but he is not satisfied with the generality of the method of neglecting the vanishing quantities.

Again, the second manuscript of October 1674 appears to be immensely important; especially as it contains the groundwork of some of the later manuscripts. Judging by the little that is given of it, it would seem to be most desirable that fuller extracts, at least, should have been given. It is a matter for remark that this manuscript is a long essay on series. Can this possibly have had anything to do with the fact that it is not given in full?

(TO BE CONTINUED.)

J. M. CHILD.

DERBY, ENGLAND.

CRITICISMS AND DISCUSSIONS.

MECHANISM AND THE PROBLEM OF FREEDOM.

I.

Men's opinions are far more commonly the result of the general presuppositions and prejudices of the age in which they live than the outcome of a rational process. Thus men believe whatever fits in with their general view of life and dismiss without a hearing anything which conflicts with it. In this age of science the scientist has become the arbiter of all questions, and his view is commonly accepted as authoritative. Hence problems which he refuses to examine, e. g., the question of the existence of ghosts, are at once relegated to the realm of superstition. Now there is some danger of freedom being placed among such problems. An indication of this is found in the following words of Haeckel, which represent the attitude of many contemporary scientists and psychologists toward the question of freedom: "The great struggle between the determinist and the indeterminist, between the opponent and the sustainer of the freedom of the will, has ended to-day, after more than two thousand years, completely in favor of the determinist. The human will has no more freedom than that of the higher animals, from which it differs only in degree, not in kind.... We now know that each act of the will is as fatally determined by the organization of the individual and as dependent on the momentary condition of his environment as every other psychic activity."[1] This view has won its way by its scientific prestige, and has been eagerly accepted by many who have never examined the evidence for freedom, but who nevertheless smile indulgently at those who are still so benighted as to believe in it. Therefore, in view of the great popularity and influence that Haeckel has enjoyed it seems profitable to examine the question of the relation of mechanism and freedom with reference to his specific teaching.

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[1] *The Riddle of the Universe,* p. 130f.

There are two main principles on which Haeckel bases his system: the doctrine of evolution, and the "law of substance." The doctrine of evolution furnishes the principal evidence for his denial of a spiritual principle in man and together with the "law of substance" leads to his mechanistic determinism. We shall therefore first consider the question whether the existence of a spiritual principle is precluded by evolution or by any other arguments suggested by Haeckel. We shall then proceed to the question of the universality of the "law of substance" and to the problem of its relation to mechanism, and finally examine briefly the adequacy of mechanism itself as a philosophic explanation of the universe.

II.

Haeckel never tires of citing facts in support of the doctrine of evolution. Since this doctrine in some form or other is now almost universally accepted as true we need not stop for an instant to inquire concerning the adequacy of this evidence. The only question for us to consider is whether the doctrine of evolution inevitably leads to the reduction of mind to matter. Put very simply Haeckel's argument for this conclusion is that since man developed from the lowest forms of life there is no reason to attribute to him a separate immaterial or spiritual principle not found in the forms of life from which he originated. Many objections to this argument at once suggest themselves to the thoughtful reader. In the first place it takes for granted the old scholastic idea of rigid continuity, according to which nothing new can ever arise. Now there are grave difficulties in this view, but even waiving these for the moment, Haeckel's conclusion by no means follows. Rather, the doctrine of continuity, if strictly held, would force him to read into the life of the lowest organism all the complex processes and meanings which have been evolved in the highest forms of life. For if evolution were rigidly continuous the very fact that certain phenomena, such as sensation and will, have developed in the later stages of the evolutionary process would show that these phenomena were implicit in the earlier forms. Thus Haeckel would be compelled to understand the protozoon in the light of man, rather than to reduce man to the level of the protozoon. He indeed seems sometimes to do this, and with an extraordinary anthropomorphism bestows elementary will and elementary emotion upon even inanimate matter.[2] If he held consistently to this view his final system would be in the nature of a theological hylozoism rather than a strictly mechanistic determinism. However, this reading of continuity, this attribution of man's processes to the lower forms of life, is misleading, as it involves what Baldwin calls "the fallacy of the implicit." As a matter of fact, if progress is genuine, new processes and new meanings must arise which cannot be interpreted in terms of the lower stages. Thus even though life has arisen from the inanimate, and consciousness from the unconscious, yet they involve meanings and processes which cannot be expressed in terms of the stages from which they arose. It is fallacious either to deny these new meanings and attempt to reduce them to earlier stages, or to read them back as implicit in the earlier stages. Hence the doctrine of evolution in no wise militates against the spiritual nature of man. Rather it leads us to expect man's nature to be higher or more developed than the merely physical or the merely biological.

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[2] Cf. *infra*, p. 304.

In addition to the argument drawn from evolution, Haeckel adduces several other considerations in support of his denial of the spiritual nature of man. He brings forward the evidence of experiments which have shown that various functions of the soul, such as speech and sense images, are connected with definite areas of the cortex of the brain and disappear when these areas are diseased or destroyed.[3] Again he calls our attention to the close connection between man's higher cerebral functions and purely physiological processes—a connection especially plain in the case of emotions.[4] He also emphasizes facts concerning the individual's development which, in his opinion, indicate that the soul originates, grows, and decays with the body.[5] Finally he points out that we never find a single instance of a spiritual principle unconnected with a physical substrate.[6]

Now although all the above arguments show clearly that there is some relation between mind and body, yet they do not succeed in reducing mind to body. The facts can be read as easily the other way. As a matter of fact, we are as conscious of the influence of mind on body as of body on mind. It is true that illness or various physical causes affect man's mental processes, but it is also true that man's mental processes affect his physical condition. Indeed every act of will is evidence of the power of mind over matter. Now to this Haeckel might retort: "What you call will is merely a certain functioning of a physiological organism. I can even disclose to you, with my microscope, the minute structures in the brain by which willing takes place." But even if this last contention were granted it would not prove Haeckel's point. The brain with its various structures may be the instrument of mind's expression without being the *cause* of mind.[7] Moreover the very facts of pathology which are cited by Haeckel to show the dependence of mind on matter are used by Bergson to prove that mind cannot be located in the brain nor determined by it.[8] Furthermore in this controversy concerning the relation of mind and body, the idealist can always go back to Berkeley's position and retort, "The brain, the nervous system, etc., to which you attempt to reduce mind, are known only as ideas of mind, and cannot be proved to exist apart from mind."

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[3] Cf. *Last Words on Evolution*, 98f; *The Riddle of the Universe*, p. 204.

[4] Cf. *The Riddle of the Universe*, pp. 127, 204.

[5] Cf. *The Riddle of the Universe*, Chap. VIII.

[6] Cf. *Ibid.*, pp. 90, 91.

A further weakness in Haeckel's arguments is that they often betray a total misunderstanding of his opponents' position. They are all directed against the existence of a separate, immaterial substance or soul. Most idealists, however, regard the soul as activity or functioning, rather than as substance. They do not insist on the separateness of the psychic principle or on the existence of any disembodied spirit, but rather on the fact that man's activity cannot be explained in purely physical or physiological terms. Suppose it be granted to Haeckel that the soul is but the "sum total of physiological functions," yet the problem of the activity of the soul is not thereby solved. Consciousness is a fundamental fact of experience, and it cannot be explained by being set aside or labeled an epiphenomenon. Therefore the materialist must explain not only how the body reacts, but how it is conscious, how it thinks, evaluates, loves, struggles, and sacrifices. It is indeed questionable whether this activity can be interpreted in purely biological or physiological terms. The so-called body becomes equivalent to the mind and demands the same sort of an explanation.

We conclude then from this discussion that Haeckel's reduction of the psychical to the physical is not valid, and we turn to an examination of the "law of substance"—the second main support of Haeckel's system.

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[7] Cf. Schiller, *Riddles of the Sphinx*, pp. 293ff; Bergson, *Matter and Memory*, pp. 299ff; James, *Human Immortality*, pp. 7-29.

[8] Cf. Bergson, *Matter and Memory*, Chapter II.

III.

The "law of substance" is a combination of the well-known scientific laws of the "conservation of matter" and "the conservation of energy." According to Haeckel, these laws are but two aspects of one great cosmic law, since they relate to the two inseparable attributes of substance. In passing it may be noted that little is gained by this combination of the two laws, since Haeckel's unknown substance is incapable of showing concretely the relation between matter and energy. Haeckel regards this law as the one great eternal cosmic law. On what evidence then can he base its validity?

The evidence for the law adduced by Haeckel lies in the realm of scientific experiment. Thus the law of "conservation of energy" rests on the fact that many experiments have shown that when one form of energy is changed into another, it may be reconverted into the original form of energy with only a slight loss due to the escape of part of the energy into an unavailable form. Similarly the law of the "conservation of matter" rests on experiments which have demonstrated that the weight of a substance does not change throughout a series of chemical transformations. Moreover no experiments have given any indication of the creation or destruction of matter or of energy, and the generalization of a great number of phenomena under these laws has been indeed a great achievement of science. Yet it is one thing to regard these laws as useful generalizations for the purposes of science, and quite another to erect them into ontological and absolutely universal laws. Against this latter proceeding, which is that of Haeckel, an emphatic protest must be made. There are three grounds for this protest: (1) the laws have never been proved to hold exactly in any field; (2) the fact that the laws appear to hold in one or two fields is no justification for the assertion that they must hold in all fields; (3) experience can never *prove* the absolute universality of any law.

In the first place, it is manifestly impossible to prove that the laws hold exactly in any field, since the inaccuracy of scientific instruments is such that *small* differences might pass unnoted. Furthermore there are always extraneous circumstances which must be taken into account in an appraisal of the results of an experiment. A scientific result is always an approximation, and the scientific law states what would take place under ideal circumstances rather than what occurs in any concrete situation.

In the second place, the fact that the laws appear to hold true within certain fields of our experience does not show that they *must* hold in all fields. Thus the demonstration of the laws in the case of physical and chemical changes would furnish no proof of their applicability to the relation between the physical and the psychical. It is at this point that Haeckel's assertion of the universality of the law depends upon his reduction of the psychical to the physical. Since this is not valid, he is not entitled without more ado to extend the application of the law to the psychical realm. The application of the law here must rest upon experiments showing that a certain amount of physical energy can be transformed into a definite amount of psychical energy and reconverted into the original amount of physical energy. Manifest difficulties stand in the way of such experimentation, but until something of the sort is carried out there is little significance in speaking of the psychical life as a form of energy. To do so merely covers up the fact of our ignorance concerning the relation between the psychical and the physical. Now apparently Haeckel himself is aware of some of the difficulties in the way of regarding the psychic as a form of energy since in his last work, contrary to many of his previous assertions,[9] he explicitly teaches that the psychic is a separate attribute of substance, coordinate with matter and energy.[10] If this be admitted, however, the psychic must demand its own law of the "conservation of the psychic," if it is to come under the "law of substance." In any case, the important point for our purpose is that until the law is proved to be valid in the psychical field it furnishes no ground for a denial of freedom.

Our last objection needs no justification, as it is a philosophic commonplace that laws resting on experience can be universalized only by means of the supposition of the uniformity of nature. This uniformity, however, cannot be proved by experience without the assumption of its own existence in the attempted proof. Thus the observation that the laws apparently hold in a comparatively few instances within the narrow range of our experience is no proof that they have always held and will always hold throughout the length and breadth of the universe.

We have seen reason to question the dogmatic assertion of the universality of the "law of substance." Yet if it be admitted for the sake of argument that the law is universal and necessary, it by no means follows that this law alone gives an adequate account of reality and a solution of all its riddles. The law is an abstraction; it is purely quantitative, and as such leaves out of account the qualitative aspects of the universe. Thus although the *amount* of matter and energy in the universe remain constant, changes in their form or in their combination bring about new qualities not reducible to mere quantity. Take, for example, the case of a chemist who mixes together two elements in a new combination. Their weight, as a measure of their quantity, remains the same, but this quantitative equality in no wise explains or describes the new odor, the new color, or other new properties possessed by the compound. These qualitative aspects, however, are certainly part of reality, although they cannot be described by the law of substance nor comprehended in a system which uses this law as the solution of all its problems.

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[9] Cf. *The Riddle of the Universe*, p. 220; *Anthropogenie*, p. 941.

[10] Cf. *Die Lebenswunder*, p. 185.

The "law of substance" is for Haeckel but a necessary consequence of mechanical causation. In fact the two for him are identical.[11] Yet the relationship does not appear to be as simple as he would have us believe. The law of substance, he tells us, is a consequence of mechanical causation, yet his proof of the latter rests largely on his supposed proof of the universality of the former. Now the law of mechanical causation, involving the equivalence of past and present, might lead naturally, though perhaps not inevitably, to the "law of substance." On the other hand the "law of substance" does not necessarily involve mechanical causation. It does indeed preclude spontaneity, but it would be as compatible with teleology as with mechanism, since it says nothing concerning the origin of changes in matter or energy. The amount of energy and matter in the universe might remain constant if their changes were due to a desire for a future state as well as if they were due to a past stimulus. Thus even the universality of the "law of substance" would not prove the universality of mechanism. The latter theory must stand on its own feet and be accepted or rejected on its own merits.

IV.

Haeckel declares that mechanical causation explains all phenomena. To quote his own words: "The great abstract law of mechanical causality, of which our cosmological law—the law of substance—is but another and a concrete expression, now rules the entire universe as it does the mind of man; it is the steady immovable pole-star whose clear light falls on our path through the dark labyrinth of the countless separate phenomena."[12] "The monism of the cosmos which we establish thereon proclaims the absolute dominion of 'the great eternal iron laws' throughout the universe. It thus shatters at the same time the three central dogmas of the dualistic philosophy—the personality of God, the immortality of the soul, and the freedom of the will."[13]

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[11] Cf. *The Riddle of the Universe*, pp. 215, 366.

Before accepting Haeckel's conclusion concerning freedom the adequacy of mechanism itself must be examined. Of the many objections which might be made, and which have been made, to universal mechanism, we shall confine ourselves to the following: (1) the universality of mechanism cannot be proved; (2) the universality of mechanical causation would not, as Haeckel would have us believe, necessarily preclude purpose and rational or ethical freedom; (3) mechanism by itself fails to give a satisfactory account of experience as we actually know it.

We contend that mechanism cannot be proved. Experience cannot show that mechanical causation is universal and necessary, and reason does not disclose any logical necessity for insisting that every aspect of reality shall be explained by reference to the past. On the contrary, the concept of mechanical causation is full of difficulties which force the mind beyond it.[14] The universality of mechanical causation is indeed a methodological postulate of science, but not necessarily a universal principle of reality. Haeckel makes many dogmatic assertions to the effect that mechanism is universal, and that even the will is absolutely bound by causal law. Thus the will is, he declares, the necessary outcome of heredity and environment. Yet obviously he cannot *prove* that such is the case. He cannot prove that A did a certain act because A had a certain heredity and a certain environment, and that A could not have done anything else. Indeed, in the case of human activities so many complex conditions occur that it is practically impossible to isolate any set of conditions in such a way as to establish a uniform series of cause and effect. Hence the establishment of causal connection (quite apart from the question of its universality and necessity) is in such cases a task for the future, rather than an accomplished fact. The possibility therefore remains that the mental life may resist such causal treatment. An indication in this direction is found in the comparative lack of success of psychology in the use of scientific methods found fruitful in other fields.

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[12] *The Riddle of the Universe*, p. 366.

[13] *Ibid.*, p. 381.

[14] For a careful analysis of causation cf. Taylor, *Metaphysics*, pp. 158ff; Ward, *Realm of Ends*, pp. 273ff; Bergson, *Time and Free Will*, pp. 199-221.

In the second place, even though mechanism were proved to be universal, this would by no means preclude the possibility of purpose, of value, and of rational or ethical freedom. Haeckel's absolute denial of all distinctions of value is evident in the following quotations: "As our mother earth is a mere speck in the sunbeam in the illimitable universe, so man himself is but a tiny grain of protoplasm in the perishable framework of organic nature."[15] "Our own 'human nature,' which exalted itself into an image of God in its anthropistic illusion, sinks to the level of a placental mammal, which has no more value for the universe at large than the ant, the fly of a summer's day, the microscopic infusorium, or the smallest bacillus. Humanity is but a transitory phase of the evolution of an eternal substance, a particular phenomenal form of matter and energy, the true proportion of which we soon perceive when we set it on the background of infinite space and eternal time."[16] From Haeckel's point of view, indeed, neither man nor the bacillus can have any value for the "universe at large," since there is, in his opinion, no purpose whatever in the universe. All is but the result of blind forces, and even the progress of evolution is of no value to the universe.[17] Now this denial of value is explained by the fact that Haeckel always associates teleology with a separate immaterial principle. He regards it as an interruption of mechanical causation, and so feels that it is incompatible with monism. Whether or not an absolute monism of any sort is compatible with distinctions of value and with freedom, at least it is plain that the denial of teleology does not necessarily follow from the establishment of mechanical causation. Mechanism, as many teleologists tell us, may be the instrument of purpose. Far from being antagonistic to teleology it alone makes teleology possible. Without it purpose would be impotent. For example—to take an analogy from human life—man can utilize natural processes for the carrying out of his purposes only in so far as he can rely upon their mechanical uniformity. Even a machine is an embodiment of purpose. It works in a mechanical way, but its construction can be explained only in terms of purpose. From this point of view, teleology is not an external principle opposed to mechanism, but rather is immanent in all natural processes, and includes and transcends their merely mechanical aspects. The processes of the universe are describable in terms of mechanical causation, but these series of mechanical changes are what they are by reference to their value for the whole.

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[15] *The Riddle of the Universe*, p. 14.

[16] *The Riddle of the Universe*, p. 244.

[17] Cf. *Ibid.*, Chap. XIII.

Again we object to mechanism taken as a sole explanation of the universe on the ground that it fails to take into account many facts of experience. Although supposed to be the direct outcome of an acceptance of evolution, mechanism has been unable to give a satisfactory explanation of evolution itself. Furthermore mechanism cannot explain the existence of values, purposes, and ideals, and many other aspects of reality.

Bergson, perhaps better than any one else, has succeeded in proving the first point. In his careful examination of theories of evolution, he shows how mechanism is forced to take refuge in a miracle to account either for the successive production and preservation of millions of minute variations in the same direction, or for the complementary changes of the various parts of an organ necessary for the preservation and improvement of its functioning. Moreover this same miracle must be repeated innumerable times as the same change has taken place in many different lines of evolution.[18] Furthermore, in every explanation of evolution, terms such as adaptation and struggle for existence occur, but these are not mechanistic terms, since they imply purpose, ends, value. The mechanist holds that all achievements of evolution are merely results of external and internal forces, which are absolutely blind. Yet if such is the case, why is the organism said to struggle for existence? Haeckel himself, indeed, often finds a place for the action of internal forces and declares that the movement of molecules is due to an inner will. "Even the atom is not without a rudimentary form of sensation and will, or, as it is better expressed, of feeling and inclination—that is, a universal 'soul' of the simplest character."[19] The term "inclination" suits Haeckel's purpose by its vagueness, but if it is at all comparable to will it implies a reaching for the future which is not explicable as merely the result of a previous force. To do justice to this "inclination" Haeckel would be forced beyond his rigid determinism.

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[18] Cf. Bergson, *Creative Evolution*, pp. 62-76.

[19] *The Riddle of the Universe*, p. 225.

The discussion of the inadequacy of mechanism as an account of evolution has led directly to our second criticism: that mechanism fails to do justice to the existence of values and purposes which are present not alone in our inner experience but which find an outer embodiment in the great achievements of civilization.[20] Surely the painting of a great picture, the writing of a drama, or the founding of a college cannot be accounted for as the result of purely natural forces. Now the mechanist, of course, does not attempt to deny the presence and power of ideals in human life. His contention is simply that these ideals themselves are the result of purely mechanical forces. Will, Haeckel says, is absolutely determined.[21] Thus, according to Haeckel, the psychologist can trace the behavior of the self to causes in preceding conditions much as the physicist traces causal connections between the motions of stones. Haeckel, however, overlooks the fact that at this point we happen to be in a peculiarly favored position. We *can* see the action from within as well as from without, and as we do so we discover a process of determination differing profoundly from the mode of determination described by the scientist. In our own case our action cannot be understood apart from our ideal. This ideal, although due to preceding conditions of one sort or another, does not act upon us as an external compelling force, but influences us through the appeal it makes to our own interests. It is an ideal for us because we ourselves select it, and not because it is forced upon us by any external force. But this process of the selection of an ideal, or of evaluation, is distinct from any process found in the purely physical world and is not describable in mechanistic terms.[22]

Haeckel himself grows eloquent over the ideals of the good, the true, and the beautiful, and urges us to put these ideals before any false ideals promulgated by superstition. Such exhortations, however, have apparently little place in an absolutely mechanistic scheme where each self is absolutely determined by his heredity and environment. Haeckel becomes indignant over what he regards as superstitions, yet he should recall that all superstitions as well as the despised dualistic philosophy are, on his scheme, natural products and therefore as necessary as his own monistic utterances.

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[20] Cf. Bosanquet, *The Value and Destiny of the Individual*, pp. 109ff.

[21] Cf. *The Riddle of the Universe*, p. 131; *History of Creation*, Vol. I, p. 237.

[22] For a clear description of the distinction between personal and mechanical determination, cf. Ward, *Realm of Ends*, pp. 179ff.

Furthermore the ideals of the good, the true, and the beautiful must be, for Haeckel, purely human ideals, since no values exist for the universe. But if man himself has as little value as Haeckel gives him, it is strange that he should regard human ideals as worthy of reverence and worship.

A final word must be added to our criticism of mechanism. The theory of mechanism itself is not, as Haeckel must believe, a purely natural product. It is due to the organizing activity of man's intelligence and could not exist without it. Haeckel regards this unifying and critical faculty of man as due to the "concatenation of presentations."[23] Yet the mere concatenation of presentations could never of itself lead to the criticism and combination necessary to bind together these various sensations under the law of causation. This unifying of experience demands, as Eucken has so clearly shown, that man be able to separate himself from the chain of nature in order to combine and order the presentations that come to him. Hence the formulation of the theory of mechanism is a fact which mechanism itself fails to explain, and the very existence of the *theory* is evidence of its own inadequacy as a final explanation of all facts in the universe.

Our examination of Haeckel's philosophy has shown the lack of cogency of his denial of freedom. While this in itself furnishes no evidence for the reality of freedom, it at least frees us from many objections that are commonly raised against it. It indicates that the problem cannot be disposed of in so summary a manner by science, and thus affords ground for those who in the twentieth century, in spite of Haeckel's dictum, maintain the possibility of freedom.

GERTRUDE CARMAN BUSSEY.
GOUCHER COLLEGE, BALTIMORE, MD.

DETERMINISM OF FREE WILL.

WITH REFERENCE TO THE PRECEDING ARTICLE.

There is a strange confusion about mechanicalism and freedom of will which seems to have been constructed by our theological school of educators on the basis of a misinterpretation of philosophical thought, and errors thus derived are still perpetuated.

The idea of the will is perhaps the fundamental conception of ethics, and an important item for moral purposes is the freedom of an acting person. But "free will" is nothing mysterious nor incredible; it is that condition of a will which is not hindered by compulsion. He is free who acts on his own account, according to his own character, and is not interfered with by external circumstances which would make it impossible for him to act as he wishes. A man under compulsion is not responsible for his action; for his act is the act of some one else, or is due to the circumstances which force him against his own will. The external circumstances may be ever so indirect and may be reducible to fear. A man threatened by the consequences of the results of his act is no freer than a man who is directly forced into acting contrary to his will by facing the revolver of a highway robber. If an act is committed because the acting person wishes the act and also willingly accepts all of its consequences, it is and ought to be considered an act of free will, and there is scarcely any thinker who would not admit this definition of free will.

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[23] Cf. *The Riddle of the Universe,* pp. 121f.

Is there any one who denies that the act of a free will, as here defined, is as much determined as any other event in this world in which we live? If the free act of a man is really the result of deliberation and if it is performed according to the nature of the actor's character, the result of this decision will be as necessary as the act of an unfree man who acts under compulsion according to motives of fear or any external force. Determinism is a general feature of the world which expresses the truth that the law of causation remains unbroken. According to the law of causation, everything is determined, even the act of a free man.

Yet there are, or rather have been, some theologians who believe in free will, not as free will necessarily must be, viz., an unhampered will, but as a *carte blanche* or *tabula rasa*, a cause that is not caused, or as a determinant which on its part is undetermined, which is free in the sense that it is unformed, or a factor that is somehow an exception to causation and not the product of the efficacy of causation. They think that a man is not responsible if his actions are determined or determinable and can be predicted, just as in moving pictures only such consequences will happen as are on the films, and the man who knows the film would naturally and necessarily be able to tell what is going to happen in the next moment. What an undetermined will is or would be, has never as yet been clearly described; it is only declared to be an exception to the law of causality, and being undetermined seems to be as much a mere chance product as the haphazard cast of dies, in which case of course the actor could no longer be regarded as responsible for a deed not determined by himself.

The truth is that if we were omniscient we could predict the history of the world from step to step just as the theatrical manager of the movies knows the next act if he knows the film that is to project it on the screen. If I know all the characters of the acting persons, I will be able to predict the outcome of their activity under definite conditions, and there can be no quibbling about it.

We must not identify necessity and compulsion. Everything is determined; and all acts are determined with necessity, even the free acts of a free man. Further it would be wrong to say that man is compelled to act according to factors which are none of his making, if he necessarily acts according to his will.

It is true that there are factors which have preceded him; among them there are factors such as have determined his character. He has been determined and his will has been given him. In this sense it is claimed that he is as unfree as any slave who is not his own master. But is that not a wrong conclusion that here too identifies necessity with compulsion? It is necessary that a man should act according to his character if he is not under compulsion. The acts of a free man are necessary because his will necessarily and naturally follows the impulses of his own character. To say that we are slaves because we follow necessarily our own instincts is simply an illogical distortion of facts. The truth is that in doing what we will we obey the behest of those factors which shaped our will. However granting that our will is not of our own making we will be obliged to confess that we are the continuation of those factors which made us, or in other words, our ancestors whose will [illegible] are ourselves in a former generation. Thus we ought to recognize openly and unhesitatingly that the whole development of the world is not a piecing together of independent [illegible] but that we are mere fragments of a continuous whole; we are phases of a prolonged history of one and the same [illegible] which may be improved, or even on the other hand weakened or diseased. Former generations have made us of the present age and future generations will be as much the product of the present generation as we are of the past. Thus if we speak of having been made by prior factors we must recognize that the factors that made us are our own existence, as we existed in former days,—yet the truth remains that a free will is definitely determined. A free will which acts in an unhampered way is as much determined as any will which suffers violence or acts under compulsion.

Miss Bussey has taken up Haeckel and criticizes him for denying freedom of will where he stands up for determinism. I do not think that Professor Haeckel will take up the cudgel and defend himself. On the other hand I grant that Professor Haeckel is an enthusiastic defender of the monistic world-conception for which he demands a strict and universal application of the mechanistic theory to all events of existence. I will not deny that Professor Haeckel sometimes accepts views which I myself would not endorse. For instance he identifies God with matter and energy while I would look upon God in contrast to matter and energy, as a religious formulation of the world order which is the ultimate *raison d'être* of natural law throughout the sphere of existence, including also the natural law that governs human society and is the basis of the rules of conduct. But this is a point which could easily be reconstructed or altered, for Professor Haeckel himself would scarcely object to it.

In order to understand Haeckel one ought to interpret his writings in the spirit in which he has written them, and ought not imply mistakes which are rather incidental points, such as Miss Bussey criticizes.

Miss Bussey in criticizing Professor Haeckel should consider that he rejects the theory of free will because he understands by free will the theological conception of an undetermined will, viz., that kind of a free will that does not exist, because it is a self-contradictory notion, an impossible and foolish conception of a misguided brain. If he rejects it he does so only in the sense in which theologians have misrepresented freedom of will as being exempt from the law of causation. And in doing so he is certainly right in the face of Miss Bussey or any one who believes in a freedom of that kind, proclaiming that it is independent of causation.

There is no need of entering into the details of Miss Bussey's discussion. Any of our readers who knows Haeckel will be able to form his own judgment. Only a few points shall be mentioned here.

The universe has certainly to be explained from the highest product its development achieves and not from its lowest beginnings. It is man that gives us the key to the appearance of the moner, while the moner will not be able to tell what its evolution will bring out in the end. On the other hand we have not solved the problem unless we trace the development of a rational being step by step in a mechanistic fashion of cause and effect. To deny it would mean to abolish science in spots. I prefer to keep my trust in science, for science to me is God's revelation. The most important step for instance is the development of reason, and it has been explained in a mechanistic sense by Ludwig Noiré when he shows how the origin of language has produced reason and not the reverse; or, to express his principle in a popular way, "We think because we speak" and not "we speak because we think." The mechanical mechanism of speech came first, and it was the mechanism of logic and grammar which has enabled us to think.

It is not a fault of Haeckel's if he holds the view that man explains the nature and significance of the moner. It proves that he is not onesided. His claim is but the natural consequence of a consideration of evolution.

The law of the conservation of matter and energy is an *a priori* law, which in its general meaning is similar to mathematical postulates. It is a demand of science and need not be proved in detail. It is a pre-supposition just as much as is the law of causation which the scientist assumes when he investigates natural phenomena. That there is a purpose in the universe is a proposition which would involve a belief that the universe as a whole is to be understood as an individual personal being after the fashion of a man. It would involve an anthropomorphic conception of God, and I doubt whether even among our theologians there are now many bold enough to take such a position. This, however, does not exclude that the universe in its processes follows a definite direction, a claim which is proved by the facts of evolution and is probably not denied by either a theistic or atheistic interpretation of the word.

Why the formulation of the theory of mechanicalism should be a fact which mechanicalism itself fails to explain is unintelligible, and why its own existence should be evidence of its own inadequacy is hard to understand, unless the notion of mechanicalism be narrowed to a limited field which does not include the entire construction of mechanicalism and its internal interrelations, such as for instance the interrelations of logical rules and conditions.

We may be able to uphold the theory of free will but we shall certainly not be able to deny the principle of determinism, and this is a blessing for the ethicist who preaches morality and claims that the freedom of will is essential for it, because if free will were indeed an exception to the law of causation and the will were undetermined and not changeable by education but remained a *tabula rasa* in spite of all attempts to change and improve it, or make it definite in the right direction, what would be the use of wasting our energies in promoting the welfare of mankind and eliminating evil influences? Let us be glad that determinism is true, for otherwise there would be no science, and principles of conduct would be a meaningless play of a misguided and erring imagination.

Haeckel apparently commits a very grave mistake. His opinions are "the result of the general presuppositions and prejudices of the age." He and many others "believe whatever fits in with their view of life and dismiss without a hearing anything which conflicts with it." Miss Bussey claims that "in this age of science the scientist has become the arbiter of all questions, and his view is commonly accepted as authoritative." In other words, we expect that science shall solve our problems, and we are prejudiced enough to bow down before science and accept its verdict. Haeckel for instance is so prejudiced that he believes in the universality of natural laws, and, says Miss Bussey, "It is a philosophic commonplace that laws resting on experience can be universalized only by means of the supposition of the uniformity of nature." It is a pity that Haeckel follows this fallacy and accepts the uniformity of nature, but the worst is that I too plead guilty. I believe not only in his "supposition of the uniformity of nature," but also in science with all that it implies, especially determinism which demands the determinedness of everything, even the determinedness of an unhampered and, in this sense, free will. I can not help it. I am in the same predicament as Professor Haeckel. May God have mercy on our souls! EDITOR.

THE BELIEF IN GOD AND IMMORTALITY.

Professor James H. Leuba, professor psychology and pedagogy in Bryn Mawr College, has undertaken to write a book on *The Belief in God and Immortality.* It is not a proof or disproof of the doctrines essential in all positive religious creeds but a study of psychological statistics as to frequency and distribution of beliefs in a personal God and a personal immortality, and he finds that upon the whole in each group investigated as to their religious beliefs, the more distinguished fraction includes by far the smaller numbers of believers.

Professor Leuba's work is divided into three parts. The first part enters into a discussion of the characteristics of a belief in a continuation after death. He begins with the savage's idea of soul and ghost, setting forth in his second chapter the origin of the ghost idea, the appearance of ghosts in dreams and visions. He distinguishes from the belief in soul-ghosts the belief in immortality which he regards as late in the development of mankind. The fourth chapter is devoted to "The Origin of the Modern Conception of Immortality," beginning with a "translation to a land of immortality." The fifth chapter enters into metaphysics, the deductions of which however are regarded as insufficient.

In later days more scientific methods have been used by relying on physical and psychical manifestations and on the historical facts on which the resurrection of Christ is taught.

In Part II the belief in the personality of God and immortality is made an object of statistical study, first (Chapter VII) among the students of American colleges. In this it has become necessary to make a distinction between the personal and impersonal conceptions of God. The eighth chapter is devoted to an investigation of the belief in immortality, including a comparison of the changes taking place during college years. Here follows a detailed investigation (introduced first by the causes of the failure to answer and the interpretation of the questionnaire) of the beliefs held by the scientists, the historians, the sociologists, the psychologists, and the philosophers, concluding with a comparison of the signed and unsigned answers. He comments on the results of his investigation thus:

"The essential problem facing organized Christianity is constituted by the wide-spread rejection of its two fundamental dogmas—a rejection apparently destined to extend parallel with the diffusion of knowledge and the moral qualities that make for eminence in scholarly pursuits."

The third part which might be considered as independent of the first two is devoted to the question of the utility of the belief in personal immortality and a personal God. Professor Leuba asks the question, "Is humanity better off with than without that belief (in a personal God and a personal immortality)? He answers: "The utility of the belief in immortality to civilized nations is much more limited than is commonly supposed.... we may even be brought to conclude that its disappearance from among the most civilized nations would be, on the whole, a gain."

It is noteworthy that his results show that the desire for immortality and the usefulness of the belief is rather disappearing with an increase of intelligence. There is an increasing tendency to disclaim any desire for immortality. This is in strong contrast to the supposition formerly quite common that even disbelievers yearn for immortality, but among the answers received to a questionnaire Professor Leuba finds even a relatively considerable number who abhor the idea of an endless continuation and he quotes a number of instances. For instance a woman thirty years of age declares that she has always felt death to be better than all else, anticipating it as the best thing life has to offer; and concluding with the sentence that death itself is a consummation devoutly to be wished.

Another letter is quoted as stating, "I feel a great dread of the possibility of having to live forever, or even again," and Professor Leuba quotes from Swinburne's poem "The Garden of Proserpina" the poet's hope of annihilation, where he says:

"Then star nor sun shall waken,
Nor any change of light;
Nor sound of waters shaken,
Nor any sound or sight;
Nor wintry leaves nor vernal,
Nor days nor things diurnal;
Only the sleep eternal
In an eternal night."

John Addington Symonds echoes the same ideas in prose. He says:

"Until that immortality of the individual is irrefragably demonstrated, the sweet, the immeasurably precious hope of ending with this life, the ache and languor of existence, remains open to burdened human personalities."

The greater stimulus for a desire for immortality comes in cases of the death of beloved persons, and the most impressive instance of this kind is quoted by Professor Leuba in the case of a widow writing to her friend, the famous Professor Schleiermacher. Quoting from Schleiermacher's *Leben* as quoted by James Martineau in *A Study of Religion,* Vol. II, page 337:

"O Schleier, in the midst of my sorrow there are yet blessed moments when I vividly feel what a love ours was, and that surely this love is eternal, and it is impossible that God can destroy it; for God himself is love. I bear this life while nature will; for I have still work to do for the children, his and mine; but O God! with what longings, what foreshadowings of unutterable blessedness, do I gaze across into that world where he lives! What joy for me to die!

"Schleier, shall I not find him again? O my God! I implore you, Schleier, by all that is dear to God and sacred, give me, if you can, the certain assurance of finding and knowing him again. Tell me your inmost faith on this, dear Schleier; Oh! if it fails, I am undone. It is for this that I live, for this that I submissively and quietly endure: this is the one only outlook that sheds a light on my dark life,—to find him again, to live for him again. O God! he cannot be destroyed!"

In commenting that the psychological state might have been quite different in Schleiermacher's friend if she had remarried. Professor Leuba says: "In that occurrence her former yearnings for another life might have been replaced by dread of the time when she would be face to face with two husbands."

Perhaps the most dignified expression of an impersonal immortality has been expressed by George Eliot in her "Choir Invisible," but the main and classical instance is the orthodox Buddhist faith, and Professor Leuba quotes at length the text from Buddhist scriptures as translated by Henry Clarke Warren, where Buddha insists on not being born again and that the present life is his final entry into Nirvana. It reads thus:

"And being, O priests, myself subject to birth, I perceived the wretchedness of what is subject to birth, and craving the incomparable security of a Nirvana free from birth, I attained the incomparable security of a Nirvana free from birth; myself subject to old age,.... disease,.... death,.... sorrow,.... corruption, I perceived the wretchedness of what is subject to corruption, and, craving the incomparable security of a Nirvana free from corruption, I attained the incomparable security of a Nirvana free from corruption. And the knowledge and the insight sprang up within me, 'My deliverance is unshakable; this is my last existence; no more shall I be born again.' And it occurred to me, O priests, as follows:

"'This doctrine to which I have attained is profound, recondite, and difficult of comprehension, good, excellent, and not reached by mere reasoning, subtile, and intelligible only to the wise. Mankind, on the other hand, is captivated, entranced, held spell-bound by its lusts; and forasmuch as mankind is captivated, entranced, held spell-bound by its lusts, it is hard for them....to understand how all the constituents of being may be made to subside, all the substrata of being be relinquished, and desire be made to vanish, and absence of passion, cessation, and Nirvana be attained.'"

It is peculiar that among scientists there was one who clung with great insistence to the belief in immortality, and this is no less an authority than the great biologist, Henri Pasteur, and he kept his religious faith and science in two different departments of his mind. He says:

"My philosophy is of the heart and not of the mind, and I give myself up, for instance, to those feelings about eternity which come naturally at the bedside of a cherished child drawing its last breath.

"There are two men in each one of us: the scientist, he who starts with a clear field and desires to rise to the knowledge of Nature through observation, experimentation, and reasoning: and the man of sentiment, the man of belief, the man who mourns his dead children and who cannot, alas, prove that he will see them again, but who believes that he will, and lives in that hope;.... the man who feels that the force that is within him cannot die."

Professor Leuba adds the following comment on Pasteur:

"I may remark incidentally upon the off-hand manner in which Pasteur divides life into two spheres, that of science and that of feeling, and apparently finds no use for logic and reason in the latter. This is a shocking example of a dangerous practice which, when carried to its logical consequence, would permit one to believe whatever he pleases. When I attempt to understand this attitude in a distinguished man of science, I can only conjecture that he treated religion as something primarily intended to comfort *anyway, anyhow.*"

Professor Leuba's book does not decide the question of the acceptability or unacceptability of the belief itself, but is simply a statistical investigation and for that reason possesses virtue for theists as well as unbelievers in helping to find out the psychological state of things as it happens to be in our present generation, and from that standpoint the book will retain its virtue whatever be the position of the reader.

SIR OLIVER LODGE ON LIFE AFTER DEATH.

Sir Oliver has always been a believer in mediumistic experience and in the spirit existence of man in the other world, and in spite of his knowledge of physics he has taken a broad stand by coming out squarely and unreservedly in showing his faith. Details of such an expression might be amusing if it were not actually sad to see a man of his significance stooping to views which otherwise prevail only in the circles of half-educated people. His son Raymond died at the front in Flanders on September 14, 1915, and the bereaved father has published a book[1] containing a summary of his own philosophical views and a record of communications received from Raymond since his death.

From this we learn that Raymond woke up in the other world and got accustomed to his new surroundings. There are seven spheres all above the earth and turning around with the earth, but there is no consecutive night and day. It is always daylight except when one desires darkness; then night spreads according to one's wishes. Raymond resides in a house which appears to be made of brick, and spirit houses form streets in which the spirits walk and move. People who have lost arms or legs develop new ones as if by a kind of natural recuperation, so he tells his parents that he has replaced a tooth, and comrades of his who had lost arms or other limbs are restored to their original natural shape, but this restoration is not quite simple and there is a kind of spirit-doctors who help with their restoration. There is a special difficulty in restoring the spiritual body if the material body has been destroyed before its regeneration in the spirit world, so Raymond gives a definite warning that dead bodies should not be cremated before father has published a book containing a summary of his own they have been restored in the spirit plane of life.

The seven spheres which are built around the earthly plane seem to revolve with it at different rates of speed, so that the first sphere is not revolving at the same rate as the second, third, fourth, fifth, sixth and seventh spheres. Greater circumference makes the revolution more rapid and this increase of rotation has an actual effect on the atmospheric conditions prevailing in different spheres. When asked for details about the nature of the other world Raymond said:

"What I am worrying about is how it is all made and of what it is composed. I have not found out yet, but I have a theory. It is not an original idea of mine. I was helped to it by words dropped here and there. People who think everything is created by thought are wrong. I imagined for a little while that one's thoughts over here formed the buildings and flowers and trees and solid ground; but there is something more than that.

"There is something always rising from the earth—something chemical in form. As it rises to ours it goes through various changes and solidifies here. I feel sure it is something given off from the earth that makes the solid trees, flowers, etc.....

"All the decay that goes on on the earth is not lost. It doesn't just form manure or dust. Certain vegetable and decayed tissue does form manure for a time, but it gives off an essence or a gas which ascends and which becomes what you call a 'smell.' Everything dead has a smell, if you notice; and I know now that the smell is of actual use, because it is from that smell that we are able to produce duplicates of whatever form it had before it became a smell. Even old wood has a smell different from new wood; you may have to have a keen nose to detect it on the earth plane.

"Old rags, cloth decaying and going rotten, all have smells. Different kinds of cloth give off different smells. You can understand how all this interests me. Apparently, so far as I can gather, the rotting wool appears to be used for making things like tweeds on our side. But I know that I am jumping; I'm guessing at it. My suit, I expect, was made from decayed worsted on your side.

"Some people here won't grasp this even yet—about the material cause of all these things. They go talking about spiritual robes made of light, built by thoughts on the earth plane. I don't believe it. They go about thinking that it is a thought robe they're wearing, resulting from the spiritual life they led; and when we try to tell them it is manufactured out of materials they don't believe it. They say, 'No, no; it's a robe of light and brightness which I manufactured by thought.' So we just leave it. But I don't say that they don't get robes quicker when they have led spiritual lives down there; I think they do, and that's what makes them think that they made the robes by their lives.

"You know flowers how they decay. We have got flowers here; your decayed flowers flower again with us—beautiful flowers."

They have not only spirit doctors but also manufacturers and can provide you with materials if you so desire. Raymond himself does not smoke, but a friend of his, a great smoker on the earth plane, demanded cigars and he got them, but only about five; and the things given him looked like cigars, but after smoking about five cigars he no longer cared for more. He changed his habit and got accustomed to a more spiritual mode of life.

Colors have their significance, and different colors have different effects upon the character of the spirits.

"There's plenty of flowers growing here, you will be glad to hear. But we don't cut them here. They don't die and grow again; they seem to renew themselves. Just like people, they are there all the time renewing their spirit bodies. The higher the sphere he went to, the lighter the bodies seemed to be—he means the fairer, lighter in color. He's got an idea that the reason why people have drawn angels with long fair hair and very fair complexions is that they have been inspired by somebody from very high spheres."

The information Professor Lodge publishes was received from the medium Mrs. Leonard through her "control" known as "Fedo."

Incidentally we find a personal remark put in brackets and in italics of which Sir Oliver is apparently the authority. It reads: "A good deal of this struck me as nonsense, as if Feda has picked it up from some sitter."

Mediums have said much nonsense in print as well as in private seances, and the spirits of dead people have distinguished themselves by silly utterances; but the recent story of Raymond's communications rather excels all prior tales of mediumistic lore in the silliness of its revelations. But the saddest part of it consists in the fact that a great scientist, no less a one than Sir Oliver Lodge, has published the book and so stands sponsor for it.

Sir Oliver Lodge is a scientist who has done much creditable work and has written a number of books which exhibit keen thought and a good grasp of his subject, his specialty being physics. The books he has written are as folows:

Elementary Mechanics; Modern Views of Electricity; Pioneers of Science; Signalling Without Wires; Lightning Conductors and

Lightning Guards; School Teaching and School Reform; Mathematics for Parents and Teachers; Life and Matter; Electrons; Modern Views of Matter; The Substance of Faith; Man and the Universe; The Ether of Space; The Survival of Man; Parent and Child; Reason and Belief; and *Modern Problems.*

CURRENT PERIODICALS.

In Vol. XIV (1915) of the fifth series of the *Atti* of the Royal Academy of the Lincei at Rome is a publication in full of the treatise *De corporibus regularibus* of Pietro Franceschi or Della Francesca which was found in 1912 in the Vatican Library by G. Mancini. To this is prefixed a learned dissertation by Mancini to show that this treatise was pilfered by Luca Pacioli in his work on mensuration, the *Divina proportione*; and a report by Gino Loria on Mancini's memoir.

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The articles of greatest interest to philosophical mathematicians in recent numbers of Vol. XVII (1916) of the *Transactions of the American Mathematical Society* are as follows. In the number for April, Robert L. Moore gives three systems of axioms for plane *analysis situs*—the non-metrical part of the theory of plane sets of points, including the theory of plane curves; Charles N. Haskins writes on the measurable bounds and the distribution of functional values of "summable" functions—which here means functions which are integrable in the generalized sense of Lebesgue; and Dunham Jackson proves in another way an important theorem of Haskins. In the number for July, L. L. Silverman discusses the generalization of the notion of the summability of a series to the limit of a function of a continuous variable; G. H. Hardy develops a new and powerful method for the discussion of Weierstrass's continuous function which is not differentiable, and allied questions; and William F. Osgood, to show that a theorem of Weierstrass for analytic functions of n complex variables is true for other "spaces" than that of analysis, lays down a general definition of "infinite regions," which includes the cases of projective geometry, the geometry of inversion, the geometry of the space of analysis, and so on.

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In the *Bulletin of the American Mathematical Society* for June, 1916, Dr. A. Bernstein reduces the number of postulates which

Huntington gave in 1904 for Boole's algebra of logic from ten to eight, and that of postulated special elements from three ("zero", the "whole," and the "negative") to one (the "negative"). An interesting and valuable address delivered before the University of Chicago by Prof. Edward B. Van Vleck on "Current Tendencies of Mathematical Research" is printed in the October number.

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The number of the *Revue de métaphysique et de morale* for May, 1916, contains a long and important article by A. N. Whitehead on the relationist theory of space. This theory is developed for a great part by help of the symbols of the author and Russell's work. The other articles in this number are by F. Colonna d'Istria (religion according to Cabanis), Léon Brunschvicg (the relations of the intellectual and the moral conscience), R. Hubert (the Cartesian theory of enumeration: on the fourth Rule of the *Discours*), and Georges Guy-Grand (impartiality and neutrality). In the July number of the *Revue* Lionel Dauriac writes on contingence and category, and tries to decide whether Kant was right or wrong in not separating the necessary and the *a priori*. Gaston Milhaud discusses the famous mystical crisis through which Descartes passed in 1619. Henri Dufumier maintains that the algebra of classes in logic only takes a systematic form if we consider it as a generalization of the mathematical theory of aggregates. F. Buisson explains "the true meaning of the sacred union." Finally, there is a necrology of Victor Delbos (1862-1916).

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In the eighteenth volume (1916) of Prof. Gina Loria's quarterly *Bollettino di bibliografia e storia delle scienze matematiche,* the most interesting articles in the first two numbers (April and June) seem to be: J. H. Graf's collection of the correspondence between Ludwig Schläfli and some of his Italian mathematical contemporaries (pp. 21-35, 49-64); and G. Vivanti's review of the late Julius König's *Neue Grundlagen der Logik, Arithmetik und Mengenlehre* of 1914 (pp. 37-39).

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THE ELECTRONIC THEORY OF MATTER.[1]

> "Wie Alles sich zum Ganzen webt!
> Eins in dem Andern wirkt und lebt."
> —*Goethe.*

THE subject of the considerations that follow is proposed as the sixth under the division of physics in the published program of this congress. Unquestionably the proposal was most timely and fortunate, for no theme of purely scientific content is more important or more central on the stage of interest or more worthy of the attention of the assembled savants of two continents. Surely it is eminently appropriate that the New World should foster the New Knowledge, should master it, acclaim it, proclaim it, and advance it.

The most obvious criticism upon any attempt to treat this theme on the present occasion would seem to be that the barrel is too large for the hoop. So far and wide reaching is the new doctrine of matter, so interpenetrative of so many remote and alien disciplines, that any half-way adequate presentation of even the most near-lying considerations would necessarily pass swiftly beyond the largest bounds to be assigned this paper and easily expand into a stately volume.

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[1] This paper, read (in Spanish) at the First Pan-American Scientific Congress (Santiago de Chile, Dec. 25, 1908 to January 5, 1909), has appeared thus far only in the *Trabajos del Cuarto Congreso Cientifico (1° Pan-Americano)*, Vol. V, pp. 1-22, Santiago de Chile, 1910.

We must begin then with renunciation. The attempt can not be made to detail but only to suggest some of the proofs (which are manifold and decisive) of the actual existence of the corpuscle, sub-atom, or electron, as the uniform elementary constituent of the visible universe is variously named. The isolated independent subsistence of this corpuscle is the central revelation of the new knowledge. It was first discovered many years ago, and proclaimed to the world as the fourth or radiant state of matter by Sir William Crookes, after whom the vacuum tubes in which the green phosphorescence accompanying the passage of an electric current were and still are named. That something called cathode rays emerged from the cathode or negative pole and moved in right lines, was proved by the shadow cast by an interposed mica cross. The English declared these rays were particles, shot out from the cathode (pole) against the inside walls of the tube; but the Germans held it was only ether waves stirred up at the pole and propagated rectilinearly. That the English were right was shown conclusively by subjecting the rays first to magnetic and then to electric attraction, whereby it appeared that they behave in all ways precisely as minute particles laden with negative electricity. Amazing is the control which the experimenter exhibits over these flying hosts of electric atoms; by deft manipulation of his infinitely fine magnetic or electric fingers he may turn the stream of corpuscles as he will and even bend it into a spiral or into a circular hoop far more supplely than one might bend the superfinest Damascus blade. But inconceivably more delicate still is the touch of the mathematical reason, whereby even the individual electron is caught in its flight and forced to tell the secret of its speed. For one may subject the flying particles simultaneously to opposite electric and magnetic influences by immersing them in two coexistent and mutually annulling fields of force, so that they fly undisturbed straight from the negative to the positive pole. These two self-destroying forces are Hev and eX, whence $v = X/H$, whereby v the velocity of the corpuscle is known when we know H the magnetic and X the electric force, both of which are readily measured. This velocity increases with the exhaustion of the tube from eight thousand up to one hundred thousand kilometers per second, which is many thousand times the mean speed even of hydrogen molecules at the highest temperature ever yet attained.

But far more wonderful and incomparably more important than this determination of a variable velocity is the determination of a constant, the most fundamental yet discovered in nature. Science itself may be defined as the eternal search for invariants amid the eternal flux of variants, and this astounding constant of which I am about to speak reminds us indeed of Plato's unwavering axis of the universe turning forever in the lap of Necessity. In the equation $Hev = Xe$, the symbol e denotes the negative electric charge borne by the individual corpuscle. If we take away the magnetic force, leaving only the electric, this latter will bend the path of the flying corpuscle as gravity bends the path of the level-aimed cannon ball into a parabola. Now Galilei has taught us the formula for the amount (s) of the bend or the fall in the time t; it is $s = \frac{1}{2}\alpha t^2$ where α is the acceleration in question. Here the acceleration is the force Xe divided by the mass m of the corpuscle; the time is the tube length l divided by the velocity v; and the distance s is the descent of the green spot at the end of the tube;

hence $s = \frac{1}{2}X(e/m).(l^2/v^2)$, whence $e/m = (2v^2s/l^2X)$, where all on the right side is known. Hereby is determined this ratio of the electric charge to the mass of the flying corpuscle, and this ratio is found to be everywhere the same (unless indeed the velocity of the corpuscle, of which it is in strictness a function, approaches that of light).

The value of this remarkable constant (for all ordinary velocities) is in the accepted C. G. S. system no less than 17,000,000 (1.7×10^7). Why is it so large? Is it because the charge e is so great, or because the mass m is so small? This question can be answered and has been answered by the exquisitely beautiful experiments of the two Wilsons (C. T. R. and H. A.) on the formation of clouds by condensation of vapor around nuclei. Not only does the water collect around particles of dust but also around any particles charged with electricity: nay more, it refuses to collect except around nuclei until the vapor reaches eightfold saturation. Now it has been found possible to free a cylinder of air from dust, and supersaturate it with vapor, and then to form in it suddenly a dense cloud by electrifying its particles with radiations from radium or still better by charging its individual molecules with electrons shot out from a metal plate played on by ultra-violet light. By attracting electrically these drops coagulated around these molecules one may suspend them in the air of the cylinder like balloons or make them fall as slowly as one will, so that their velocity of fall may be measured; and Stokes has deduced the formula for this velocity, $v = 2/9.ga^2/\mu$ where g is the known acceleration of gravity and μ the known coefficient of viscosity; hence a, the radius, and thence the size of the drop is found; and hence by measuring the amount of watery vapor deposited one finds the number of the drops and so can count the number of electrified molecules, that is the number of corpuscles, since each molecule has but one negative electron. Plainly, if by one chance in a trillion two corpuscles should light on one molecule, their mutual repulsion would dislodge them instantly.

By electrometric methods one may find the total charge of electricity on the total water, the sum of the drops, and dividing this by the number of drops or corpuscles one finds the charge *e* on each, and then on dividing this by the constant ratio there results the mass *m* of each corpuscle. These numbers turn out to be appalling in their minuteness. The charge *e* equals $310/10^{12}$ of an electrostatic unit, or one one-hundred-trillionth (10^{-20}) of an electromagnetic unit, and is the long well-known approximate value of the charge borne by an atom of hydrogen in the electrolysis of dilute solutions. The mass *m* of the corpuscle proves to be six hundred quintillionths of a gramme (6×10^{-28}), a degree of parvitude far beyond the utmost stretch of the imagination. The same may indeed be said of the atom of hydrogen, but the mass of this atom is 1700 times[2] the mass of the corpuscle. Hitherto this hydrogen atom has been conceived as standing on the remotest confines of matter, but the new knowledge shows us a still lower world of corpuscles, nearly 2000 times smaller.

At this point it may be proper to enter a caution. It is almost universal to speak of this corpuscle as of invariable mass bearing an invariable charge of negative electricity, and the calculations and experiments do indeed yield results uniform within the limits of error. But we must remember that these experiments and calculations have always treated and apparently must always treat not the individual corpuscle but millions on millions of corpuscles and atoms. The results then were only averages of countless numbers of individuals, and the constancy of such an average implies nothing at all as to the constancy or inconstancy of the individual, just as the comparative steadiness of the rates of birth, death, marriage, homicide and the like, even in a population of a few millions, implies nothing whatever as to the rate in any particular family. For all we know the range of individuality among atoms and corpuscles may be quite as great as among suns or planets or men, and this we must say even in face of the famous dictum of Maxwell, that atoms of any one substance have all the marks of manufactured articles, being all exactly alike.

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[2] Later determinations raise this number to 1830 or even 1872. M. Perrin's experiments on "visible molecules" indicate that the mass of a hydrogen atom is 1.63 quadrillionths ($1.63/10^{24}$) of a gram. Hence the mass of an electron would be $0.8/10^{27}$ gram.

Returning from this digression we must now ask what is the mass and what is the charge of electricity of the corpuscle? It is precisely here that the new knowledge calls for the profoundest transformation of our conceptions, for it derives the phenomenon of mass in the corpuscle solely from the motion of the flying charge of negative electricity. We all know that work is needed to start a body in motion, as a car even on a perfectly smooth track. For any particular body having a particular velocity v, the amount of work necessary is perfectly definite, namely, $\frac{1}{2}v^2$ multiplied by a constant, M, called the *mass* of the body. We say the kinetic energy imparted is $\frac{1}{2}Mv^2$. This supposes the motion is in a vacuum, which is never the case; in practice the motion is always in some fluid, as water or air. Then we all know that more work is needed, according to velocity. One fans oneself gently with ease, but violently only with great effort. In fact, the fluid is also set in motion as well as the body, and this calls for energy or work. How much fluid is dragged or pressed along with the body will depend on the body's size, shape and speed and on the density of the fluid. Some of the simplest cases have been studied. Sir George Gabriel Stokes has found that in case of a sphere the work done on the fluid is $\frac{1}{2}M' V^2$, where M' is the mass of a volume of the fluid half as large as the sphere (shown by Green, 1833), so that the total energy imparted is $\frac{1}{2}(M + M')V^2$. Now all motions take place in the all-pervading ether. It follows then that, if the ether itself has mass, when put into movement it must absorb energy or require work, and hence that some perhaps infinitesimal part of the mass of a moving body even in a vacuum must be due to the swirl set up in the ether. In the case of the moving corpuscle the analogy is not absolutely perfect, but exact enough to make intelligible the statement that if a conducting sphere of radius r, having a charge e and mass m, be set moving with velocity v, the energy developed in the ethereal magnetic field has been proved to be $\frac{1}{3}k(e/r).v^2$, so that the total work done is $\frac{1}{2}[m + \frac{2}{3}k(e/r)]v^2$, and the ordinary mass m is thus increased by $\frac{2}{3}k(e^2/r)$, which stands for the inertia overcome in the ether. (This k is a factor due to the crowding together of the lines of force into a plane through the sphere center, and perpendicular to the motion, and increases rapidly as the velocity becomes great.) Since e is extremely small, this increase is wholly imperceptible in case of all single bodies subject to our senses or experiment. But for the corpuscle, when r becomes inconceivably small, this so-called electric mass assumes important proportions, yea, it accounts for absolutely all the mass of the corpuscle, which must have this electric mass and need have no other at all. For Kaufmann has measured the value of e/m (or m/e) for the various corpuscles emitted with various velocities by radium; and J. J. Thomson has calculated k for these velocities. It turns out that the calculated relative increase (due to rising velocity) in the electrical mass is constantly equal to the observed relative increase in the whole mass, whence one must conclude that the electrical mass is the whole mass, for if there were any ordinary non-electrical mass, however small, it would certainly not thus increase apparently with the increasing velocity. The mass of the corpuscle is thus not located, at least in any appreciable degree, in the corpuscle itself, but only in the universal ether around it.

Imagine a sphere surface perfectly rigid but absolutely void, empty even of ether itself, a mere round hole in universal ether. If set in motion this hollow sphere would gather mass as it gathered velocity, but the mass would not be inside, it would be wholly outside, inwrought in the universal eddy set up in the infinite ether. In this sense the mass of the moving hollow sphere would be coextensive with the whole space filled by ether, and in this sense we may say the same of the mass of a flying corpuscle: it reaches throughout the world. We may imagine it as a mere needle-point from which Faraday tubes of force radiate to the utmost stars. But since the ether bound by the tubes varies as the squared density of the tubes, and hence varies inversely as the fourth power of the distance from the sphere center, it follows that the corpuscle mass is after all highly concentrated round the corpuscle core. For an easy reckoning shows that the corpuscle radius r is only about five millionths of the molecule radius, which is commonly taken as the hundred millionth of a centimeter; that is, of course, in *order of magnitude*. Hence from the surface of a molecule to the surface of a corpuscle at its center the mass intensity would increase more than a trillionfold.

It follows that one can no longer affirm with perfect rigor the principle of the conservation of mass, for the masses vary constantly with the velocities of the corpuscles. But to our gross senses even when armed with the most delicate instruments these variations might forever remain imperceptible. However, Heydweiler claims to have actually detected a difference in the joint weights of water and copper sulphate before solution and after, and Wallace holds that the mass of water is changed by freezing—highly interesting results that await confirmation. But it must not be supposed that the notion of mass itself is hereby eliminated or even reduced to greater simplicity. For all these results assume to start with the assumption that the ether itself has mass. Calling then to one's help the Faraday image of tubes of force and still more the hydrodynamics of vortex rings, one may deduce from ether-mass the mass of all material bodies; but mass itself adheres along with time and space inexpugnably in our reasonings.

Corpuscles therefore are; atoms also are; how then shall we think them related? As the corpuscle mass is only 1/1700 of the hydrogen atom mass, shall we think this smallest atom as compounded of 1700 corpuscles? There are many reasons against such a doctrine, reasons that lead one to think the number of corpuscles in the atom as always small. But shall we think the atom as in any case composed of corpuscles? There seems to be no escape from such a conception, which lies directly across the path of thought. For many experiments that cannot be mentioned here show that corpuscles are all-pervasive. Metals heated pour them forth, as do all other substances hot, and some shoot them out even when cold, as rubidium, and at fearful speed, as all radio-active substances; yea, if we had instruments fine enough we might detect them in every substance, and everywhere maintaining the constant ratio e/m inviolate. Moreover, that the corpuscle is closely related to the atom is clearly seen in the fact already mentioned that the corpuscle's and the hydrogen-atom's charges of electricity are the same.

Before trying to construct imaginatively the atom out of corpuscles we must recall that there are rays (as Goldstein's *Canalstrahlen*) of positive as well as of negative electricity, that are deflected by a magnet oppositely to the negative cathode rays. For them the ratio e/m is not constant and never exceeds 10,000, which is also its value for hydrogen ions in electrolysis of dilute solutions. It is natural to figure thus the positive corpuscle as a sphere of positive electrification, about the size of an atom. Of course such a sphere implies an equal and balancing amount of negative electricity, and this we imagine distributed throughout the sphere as equal corpuscles or units of negative electricity. Since the atom is permanent, this distribution of negative electrons in the positive sphere must form a system in stable equilibrium, and the question arises, what arrangement of the electrons would constitute such a stable system? The problem is mathematico-mechanical, and its general solution lies beyond the range of our present powers of analysis; but if we propose the problem not for space but for the plane, we may solve it and get a system of answers quantitatively different but formally analogous to those that must be rendered for threefold space.

At this point theory and experiment have joined hands in a most friendly fashion. As early as 1881 the present Cavendish professor of physics at Cambridge, stimulated by the brilliant experiments of Crookes, in a long neglected but now classical paper in the *Philosophical Transactions,* discussed the motion of a charged sphere in an electric field, thereby laying the foundations of the doctrine of electric mass. Twenty-three years later (1904) he advanced to the discussion of the equilibrium of a system of negative electric charges abandoned to their mutual attractions in a positively electrified shell.* He showed that the configuration of planar equilibrium is (in general) a regular polygon concentric with the sphere, but for six particles the equilibrium is unstable, one particle will break ranks and rush to the center, while the other five form a regular pentagon. Similarly if there be seven, eight or nine; if there be ten, three will form an inner equilateral triangle, and so on up to seventeen, when one of the inner ring will again break ranks and fly to the center, leaving an inner ring of five, and an outer ring of eleven. (Four corpuscles cannot be in planar equilibrium at rest, but only when the four are in rapid rotation. At rest they are at the corners of a regular tetrahedron whose edge equals the radius of the sphere. Six corpuscles are balanced at six corners of a regular octahedron.) When the number reaches thirty-two, the three-ring system becomes unstable, again a particle seeks the center, leaving an inner ring of five, a middle ring of eleven, an outer ring of fifteen. Looking at it another way one may ask how many must be put inside a ring of *n* to make it stable. The answers are: for 5, 0; for 6, 7, 8, each 1; for 9, 2; for 12, 8; for 13, 10; for 15, 15; for 20, 39; for 30, 101; for 40, 232. You see how rapidly (as the cube of *n*) the inside corpuscles multiply as the outer ring increases in number. The structure must be compact, densely peopled toward the center, not hollow like a shell.

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* For an additional note see page 480.

The whole scheme of numbers has been worked out by J. J. Thomson (*Philosophical Magazine,* 1904) mathematically, and a beautiful experiment first made for another purpose by the American Mayer, afterwards under other forms by Wood and Monckmann, confirms his results. On a water surface any number of small equally magnetized needles are made to float by being thrust through cork discs, only like poles being above the water. These repel each other like the negative electric corpuscles. The sphere of positive electrification is represented by a magnet hung above the water, the opposite pole pointing downward. This holds the magnets in groups in stable equilibrium, and the arrangements of the magnets actually observed agree excellently with the arrangement required and predicted by mathematical analysis. Provisionally then, we may proceed on the working hypothesis that the atom is a system of corpuscles composed of a number of concentric sub-systems all held in stable equilibrium by an enclosing sphere of positive electrification. This conception may be somewhat vague and may hereafter require modification, but it is far clearer and more precise than was possible a few years ago and constitutes a notable advance in physical theory. We have spoken of the configuration as stable, but this stability must not be understood too strictly nor as always equally rigid. Since we may have an outer ring of five, six, seven or eight with only one at the center, it is plain that if there were seven in equilibrium, neither an increase nor a decrease of just one would disturb the system much; but if there were sixteen the arrangement would be one of two rings, eleven in the outer, five in the inner; take away one and the arrangement persists with only ten outside; but add one and the two-ring system is no longer stable, a particle goes to the center, the rings remain unchanged, a three-ring system is formed. A better though more complex example is afforded by the group of eleven, ranging from 58 to 68 corpuscles (Thomson); all these have five rings, thus, in order:

19	20	20	20	20	20	20	20	20	20	21
16	16	16	16	17	17	17	17	17	17	17
13	13	13	13	13	13	13	14	14	15	15
8	8	8	9	9	10	10	10	10	10	10
2	2	3	3	3	3	4	4	5	5	5
58	59	60	61	62	63	64	65	66	67	68

Here we see that if a corpuscle be injected into the 58-system it produces the least possible disturbance, place is made for it in the outmost ring and the others remain as they were, a 59-system results. But if a corpuscle be added to this it can find no place in any ring but the central; it must find its own way to the center ring or else dislodge a corpuscle from the outer ring, which corpuscle will then dislodge another from the next and so on till a corpuscle is finally dislodged into the innermost ring from the one next to it. Still another corpuscle may be injected and another and another, profoundly altering the original arrangement but preserving the outer ring unbroken.

So it goes on, the center becoming denser and the outer ring more stable till the total number 67 is reached, having the arrangement 20, 17, 15, 10, 5. Here the central mass, though it may still make room for another corpuscle, is very steady and stubborn, so that now when another corpuscle is injected, the outer ring yields, absorbs it and now has 21. Accordingly this 67-system is like that of 58, most stable, changing least from its original form. The group of arrangements from 59 to 67 corpuscles forms a series all having twenty in the outer row, the stability of each system increasing up to the last, after which a new group begins with similar properties but only eight members. Now these corpuscles are units of negative electricity. As the number of these inside the atom increases, the outer ring remaining the same, the stability, measured by Q, the work necessary to disperse all the units infinitely apart, increases; the more inside the more firmly the outside ring is held. Hence the 59-system will be least stable, a unit would easily fly off leaving only the preceding 58-system. If we suppose this 59-system neutral, on losing this negative unit it becomes comparatively at least electro-positive; in fact the most strongly electro-positive of this series of arrangements. The following members must lose more and more negative units in order to become electro-positive as this 59-system. The 67-system is charged with the utmost negative excess and so is most electro-negative or least electro-positive. The outer ring of 20 will in fact admit no more negative units inside, but on still further addition a new outer ring of 21 is formed and a new series begins with a highly electro-positive system of 68 and again runs down to an electro-negative system of 77, in each of which the outer ring is 21. Herewith then we attain a new notion of valency. For the 59-system has only just sufficient corpuscles inside to maintain its outer ring of 20; the fifty-ninth in the system, the twentieth in the ring, might easily break away leaving only 19 outside and the atom positive from the loss of the negative unit. But it could not remain positive for it would draw to it another corpuscle and so restore its ring of twenty, and this process might be repeated. But as many as 8 negative corpuscles might be injected into the ring of 20, raising the total number to 67; hence we may say this 59-system corresponds to an atom of valency 8 for a negative unit charge and of valency 0 for a positive charge, since it could not assume permanently the positive unit charge. Consider next the 67-system. It is impossible to drive a single negative unit within the 20-ring; if one collide with the atom it stops in the outer ring making 21, but this ring is very unstable and would easily lose this electric unit; hence we may say this system has no negative valency or a negative valency equal to 0. However, this same 67-system might lose one, two or three. . . .or even eight atoms, reducing its negative, i. e., raising its positive, charge, though with harder and harder work; it could not lose more without changing its outer ring and passing into another series. Hence we may say that its positive valency is 8, just as its negative valency is 0. It is needless to dilate upon the intermediate members. Similar considerations show that we may arrange them thus:

No. of corpuscles . .	59	60	61	62	63	64	65	66	67	68
Valency .	+0	+1	+2	+3	+4	—3	—2	—1	—0	
	—8	—7	—6	—5	—4	+5	+6	+7	+8	
	Electro-positive					Electro-negative				

Such series are actually found among known chemical elements. Such are helium, lithium, barium, boron, carbon, nitrogen, oxygen, fluorin, neon, and neon, sodium, magnesium, aluminum, silicon, phosphorus, sulphur, chlorin, argon. Of course it is not meant for a moment that any such planar arrangement is actually present in chemical atoms; their corpuscles must be arranged in tridimensional space; but it can hardly be doubted that relations analogous to the foregoing, only more complicated, must characterize spatial as well as planar arrangements. If we call the tendency of a system to shed a corpuscle *corpuscular pressure* (*outward*), then it appears that this pressure changes abruptly at the end of each series; thus at 58 the pressure is very low, at 59 very high; at 67 low; at 68 high: it falls through the electro-positives down to and through the electro-negatives. We might then define positive valency of an electro-positive (or negative) as the greatest number of corpuscules it can lose without abrupt fall in corpuscular pressure; the negative valency of the electro-negative is the number it can gain without sudden rise of corpuscular pressure. Upon these definitions and conceptions has been erected a most plausible theory of chemical combination, into which we cannot enter. But one other aspect may not be passed by in silence. While no one affirms that the planar forms of equilibrium are the actual forms assumed by corpuscles in atoms, it seems hardly possible that they are not similar thereto, similar enough to allow a most important conclusion. These forms are arranged in series, and the members of these series bear striking resemblances. There are rings outside of rings, and rings outside of these, and so on. Thus around a central one there is a ring of 6, giving 7; and around this a ring of 11, yielding 18; and around this a ring of 15 making 33; and around this a ring of 18, making 51 in all; and around this a ring of 21, which makes 72; and around this still another of 24, or a total of 96.

It seems impossible that atoms consisting of these or any such systems of corpuscles should not have many likenesses in property. We are reminded of a determinant of a definite form, whose degree is raised by bordering it successively by parallel lines above and below, on the right and left. The general properties of the determinants remain the same. If then all possible forms of equilibrium should actually be realized as chemical elements, of necessity these elements would fall into series and in fact a twofold series which might be expressed by a vertical and horizontal alignment, the elements in the vertical rows being alike in their central rings but differing in the number of rings; those in a horizontal row being alike in the number of rings and in their outer ring but different in their inner rings. Herewith then we are lifted up to what is commonly regarded as the apex of chemical theory, the periodic law of Mendelyeev, which thus appears not as an empirical observation, however great or important, but as an inexorable necessity of the mechanical laws of configuration and equilibrium. It is most remarkable also that herewith the law is explained not only in its rigor where it is rigorous, but also in its laxity where it is lax. For there is no necessity that all the possible forms of equilibrium should be actualized; there might very well be gaps, even considerable gaps, in both the vertical and horizontal series. In that case some gap, say in a vertical row, might have next to it some actual form in a near-lying parallel line, which would thus present not an exact but only an approximate repetition of property.

Thus the electronic theory of matter yields not only a vivid idea of the necessary existence of a double system of valences among atoms, and of the probabilities and nature of chemical combinations, but also yields deductively in a highly acceptable form the confessedly highest and most significant induction yet reached in chemical theory. This conception of the atom not as an infinitesimal grain, strong in solid singleness, as Lucretius fancied it, nor yet as a vortex-filament in an incompressible friction-less ether, so sleek and slippery as to wriggle out from under the edge of the keenest knife and sharpest scissors, as Helmholtz and Kelvin conceived it, but as a highly organized community with members held together in unity in stable equilibrium, not at rest but in a system of complicated movements of inconceivable velocity, whose very swiftness itself contributes indispensably to the stability of the configuration (see p. 480),—this conception not only imparts new grandeur to physics but aligns it on the one hand with astronomy, on the other with biology and even anthropology. For we are all familiar in general terms with the planetary system and also with Bode's law, a special case, it would seem, of some principle of balancing, such as reaches from the atom to the constellation, from the star dust of a nebula to the most complex organization of human society. Indeed the principle of natural selection would seem to be hardly less operative in the world of brute matter than in the world of life.

More specifically, however, the electronic theory casts a broad beam of light on some long outstanding enigmas of astronomy. The motion of comets, presenting in the vast sweep of their tails an apparent repulsion from the sun, finds in this theory a long desiderated explanation. At this point science has to thank a large number of widely separated conceptions and personalities. It was the British Maxwell who as early as 1873 confirmed the suspicion of the German Euler (1746), that ethereal undulations must produce a longitudinal pressure along the ray of heat, and three years later the Italian Bartoli reached a similar, more general conclusion by a wholly different path. The mathematical prophecy declared this pressure to be $E(1 + r)/v$, where E is the energy and v the velocity of light, and r the coefficient of reflection of the receiving medium. But this phenomenon is so extremely subtile as long to have eluded the keenest observation, though the unerring finger of mathematics was pointed at it steadily for twenty-eight years.

At length (1901) the Russian Lebedev succeeded in detecting and even in measuring it. Two years later the Americans Nichols and Hull not only repeated Lebedev's experiment with far higher precision, but showed decisively that the observed value of the repulsion agreed within the limits of error with that foretold by the English clairvoyant Maxwell. Of course this repulsive push is inconceivably minute, and on even a very small sphere it would be imperceptible in comparison with the extremely feeble attraction of gravity. However, the latter decreases as the mass or as the cubed radius, while the repulsion decreases as the surface or as the squared radius, of a spherical particle. No matter then how much greater the attraction than the repulsion on any given sphere, as the radius decreases the repulsion must finally gain the upper hand, the particle sufficiently minute must be repelled by the light away from the sun. A particle of earth 0.00001 of an inch in diameter would hang balanced between the push and the pull; if larger it would fall, if smaller it would rise and fly away before the thrust of the light. Now we need not indeed have recourse to electric corpuscles to find particles much below this critical magnitude, and the phenomenon of cometary tails blown backward from the sun with inconceivable velocity, as by the breath of a god, is readily intelligible as resulting from the now demonstrated pressure of light.

Into the details of this matter it is impossible to enter here; suffice it that the illustrious Swedish physicist, Svante Arrhenius, has subjected the whole subject to rigorous calculation which has been in the main verified, at certain points amended, by Schwarzschild, so that the enigma of cometary motion may now be regarded as solved. It is found in fact that the maximum direct pressure at the surface of the sun is 2¾ mg. per square centimeter. While gravitation sets an upper limit to the diameter of the repelled particle (0.0015 mm.) the diffraction of light sets a lower, namely, about 0.1 the wave length of the light in question. Only particles whose diameters lie between these limits can be repelled, all others are attracted. The greatest theoretic repulsion is 19 times the attraction of gravitation on a particle of water; but the heterogeneity of solar light reduces this by nearly half, leaving as maximum only the tenfold of gravitation on a water sphere of 0.00016 mm. diameter. Since molecules fall far below this size, it appears that Maxwell's law does not apply to gases. But here again the corpuscle vindicates its great importance in cosmogonic theory. For the gases near the sun must be at least partially ionized, since its light is rich in ultraviolet rays and these provoke the radiation of ions. But these ionized gases, as proved by the Wilsons, are readily beclouded by the vapor condensing around the ions or corpuscles. The drops thus formed must be repelled by the light pressure, or if too heavy must fall toward the sun and bear away with them the negative electricity, leaving the gas positively laden. Hence the great part played by electricity in cometary phenomena.

While the second and third of Bredichin's groups of comets are easily understood as composed of hydrocarbon particles, the first group shows repulsion nineteen times as great as gravity, and the comets of Rooerdam and Swift (1893 II and 1892 I) even 37 and 40.5 times as great. Such repulsion would require a specific gravity only one-tenth that of water, but it may well be that hydrocarbon spherelets subjected to expulsion of hydrogen under intense solar heat may be turned into sponge-like carbon pellets of the levity required.

The combined conceptions of the forward thrust of light and the universal radiation of corpuscles give us a widely imaginative vision of the heavens above us. The corona of the sun with its greenish pearly light is no longer a mystery. We think of it as composed of particles near the critical sizes and hence held suspended in the sky, like the coffin of Mohammed, between the pull of gravitation and the push of the light, two forces obeying the same law of inverse squares. Its tenuity transcends all conceiving, it being five million times thinner than the head of the comet, and its whole mass if of coal would be burned upon earth in a week. Nevertheless it is something, and it assures us that an endless drizzle of still finer dust is steadily poured out by the sun into surrounding space. In perhaps six thousand billion years the sun might dissipate itself entirely. Unless time be finite it would follow that at this rate all the suns in the universe would have dissolved unending ages ago and vanished like the baseless fabric of a dream. Meantime, however, there has been integration proceeding *pari passu* with disintegration. Since the earth in flight round the sun sweeps up about twenty thousand tons of meteoritic matter yearly, it is easy to reckon that the sun must catch some three hundred milliard tons in the storm that drives forever about him. These meteorites are in fact the building stones of the suns. Of what are they themselves built? According to Nordenskjöld, they are woven together of metallic atom on atom, the universal floating dust of dissolved or dissolving worlds. An awful, a tremendous cycle!

As the sun thus inspires and expires matter like some stupendous lung, similarly it respires electricity. The cloud particles repelled by light-pressure bear away negative electric nuclei and leave a positive charge behind of nearly 250 milliard coulombs, enough to dissolve 24 tons of water. As the solar dust aggregates into meteorites it dispels its negative electrons and these are drawn in a ceaseless flood toward the sun by its positive charge. The sun is thus at once a source and a sink of electrons streaming into and from it, to and from the uttermost walls of the world.

We look aloft into the azure heavens and think to behold a sky unflecked by the minutest cloud. But reason perceives even there an eternal whirlwind of cosmic dust swirling around planets and stars and darkling suns. Well for humanity that the veil of this absorbent mist is flung abroad over the whole heavens! Else would the dome of the sky glow like a furnace, and the countenance of creation be withered and blasted. But not only do these nebulas and frozen stars shield the earth from planetary death; they are the very guardians of the universe itself (according to Arrhenius) against that heat-death (*Wärmetod*) with whose frightful specter Clausius has so long appalled the stoutest hearts of science and philosophy. For in his second law of thermodynamics the German declared that the entropy of the universe tends to a maximum while the energy remains constant. Now this entropy (of a body) is its total heat divided by its absolute temperature. It measures the amount of heat turned into a body in the process of exchange and so rendered unavailable for outward effect. The one universally observed case is that bodies at unequal temperatures tend to exchange heat till they attain the same temperature, when all effective interaction ceases. Applying this observation to the universe, Clausius declared that it was tending to such equilibrium, which would suspend all effective activity, and this universal uniform condition he named heat-death, and taught that it was inevitable, merely a matter of time — a fearful piece of reasoning, which it seemed equally impossible to accept or to refute. But the facts were against Clausius, for least of all men would he deny the past was infinite, hence the universe had had time to run down infinitely often, nevertheless it is manifestly not run down but running on still. Since it has not met the *Wärmetod* in the infinite past, neither need it do so in the infinite future. Maxwell imagined an intelligent demon sorting out the atoms in a uniformly heated gas, by opening the windows of an invisible partition for the passage of each fast-moving particle and shutting them against the slow-moving one, thus sifting them into two apartments, the one hot and therefore able to work on, the other cold. We know, however, of no such demons, though it would be rash to deny them all existence. But on the other hand molecules moving swift enough (more than 11 kms. per second) must tear themselves away from the earth's attraction, leaving behind their slower fellows, thus sorting out the two classes after the fashion of Maxwell's demons. In this way perhaps the moon has gradually lost her atmosphere. So too may the nebulas lose fast-flying molecules that wander into their outer parts and retain only the slower ones and so remain or even become cool. Meantime the fast flyers would be caught in the widespread atmosphere of some condensing star and so raise still higher its rising temperature. Whether or no this be the exact fashion in which the universal activity is maintained, we may be sure that it is maintained in some fashion, and the dreadful presage of universal heat-death that has so long oppressed the scientific consciousness may now be dismissed as the nightmare of a fevered dream.

Still other riddles of the heavens have yielded to the divination of the corpuscular theory. Since we must now think of the sun as sprinkling all space with an incessant shower of dust, clearly the earth too must be thus sprinkled. The atmosphere can no longer be thought as imperceptible beyond 100 kms., but must certainly reach an average height of 400 kms. Were it not for the electric charges with which the particles are laden, the amount of sun-dust that could reach the earth could hardly exceed two hundred tons yearly, one one-hundredth only of what actually reaches us in meteors and shooting stars. On the contrary, Nordenskjöld reckons the cosmic dust positively charged that reaches the earth at ten million tons yearly, and Chamberlain advocates a planitesimal theory that considers the planets as mainly built up of collected meteors. Be that as it may, the significance of the negatively laden particles in the higher regions of our atmosphere is beyond question. Of the luminous effects of wide-scattered dust in the air, the appalling eruptions of Krakatoa (1883) and Mount Pelée (1902), which reddened the sky for months, have furnished examples. On a far grander and more benignant scale the sun powders the upper air into unearthly radiance. Inhabitants of the polar regions have the nightly vision of the Aurora Borealis in two forms long confounded but now clearly distinguished: the one a nearly steady phosphorescent gleam swelling in arch on arch toward the apex of the sky; the other consisting of beams, of fountains of light spouting their torrents of splendor zenithward from the fluttering draperies of flame that fringe the northern horizon. The explanations of these two classes are of course not quite the same, and in their detail one must distinguish between the maximal and minimal years of sunspots; but in general one may say that the negatively laden particles shot out in perpetual tempest from the sun must beat upon the atmospheric envelop of the earth, which is speeding on as through a driving rain. It is the equatorial regions that are full exposed to this tempest. Great however as is the velocity of the particles, they do not in general pierce through this envelop, but are caught as it were in the net of the lines of the earth's magnetic force. Round these they spin in descending converging spirals toward the poles. On impinging upon strata of air as dense as in a vacuum tube they act as in such tubes and exhaust their energy in the fitful gleams peculiar to cathode rays. In case however of a maximal year of sunspots the velocity of the projected particles is enormously increased and they drive on nearly in straight lines in spite of the suction of the lines of force both in the sun's and even in the earth's magnetic field. Hence the Aurora may in these years descend even toward tropical regions. It is at once perceived that the relation between the two phenomena of sunspots and polar light must be intimate and so it is, but into this there is no time to enter.

Still another astronomical puzzle, not however of polar but of tropical observation, seems now in fair way toward solution. The inhabitants of even higher latitudes may behold morning and evening at the equinoxes a cone of light rising from the horizon and called zodiacal from its connection with the zodiac. It is certainly a luminous cloud of particles glittering in the sunlight. Some have thought it to be the last remnant of nebular dust still circling the sun like a ring. But according to Arrhenius both the zodiacal light itself and its still more perplexing *Gegenschein* are due to the same corpuscular storm that pours steadily out from the sun and down upon it from all surrounding space. Surely enough has now been said to show that the corpuscular theory has the widest-reaching astronomic and cosmogonic bearings.

May it not also have biologic significance? The problem of the origin of species on our planet has gone through a variety of phases. Linnæus held that the Infinite Ens had in the beginning created just so many species, which remained unchanged down to our own day. This rigid conception was shaken by Lamarck and others a century ago, but again restored to acceptance by the great authority of Cuvier. Finally it went down forever before the researches of Darwin and the school of evolution. More recently De Vries has detected species in the very act of transvolution, not however through the gradual accumulation of infinitesimal variations, but by finite leaps or mutations establishing a new species in a single generation. Meantime the deeper problem of the origin of life on our planet has not been advanced toward solution, unless the successive recognition of one proposed solution after another as unsatisfactory may be said to be advancing. Thinkers of the highest rank still cling to the notion of "spontaneous generation," under the compulsion of such reasoning as this: there was a time, no matter how remote, when there was no life on the earth; now there is life; therefore sometime between now and then life began to be. Others however spy out another possibility, namely, that life was imported from some other planet. The illustrious Kelvin insisted that the maxim *omne vivum e vivo* was as sure as the law of gravitation and hence was driven to maintain (1871) the hypothesis that life had been borne to our planet on some meteorite, some disrupted fragment of another world. But the difficulties that embarrass the development of such a notion seem quite unconquerable. More acceptable is the modern idea of panspermia, hinted by Richter as early as 1865. In a word, this doctrine holds that the germs of life are scattered as spores throughout all the deepest abysses of space, that they are driven by light-pressure along the sunbeams from planet to planet, from system to system, on journeys that may last for thousands or ten thousands of years. The intense cold of the interstellar spaces need not chill them to death since they are unaffected by a bath of liquid hydrogen (—252° C.). They need not dry out, they need not suffer any destructive chemical change, as oxidation, on their solitary flight from world to world, since evaporation and chemical processes are suspended in that Lethean flood. They need not be too large for the fingers of the light to push before it, since they have been discovered having diameters between 0.0002 and 0.0003 mm., and are doubtless often much smaller, magnitudes well within the grasp of the sunbeam. But how could they be lifted up into the higher regions of the planetary atmosphere, and there surrendered to the propulsion of the waves of ether? Currents of air might lift them a hundred kilometers from the earth, but could never release them from the atmospheric envelope. Here again one must invoke the omnipotence of the electron. The negative-laden sundust that kindles the Aurora fires in the upper air must also beat upon these spores and charge them with electricity. So charged they must powerfully repel each other in every direction and some would be launched outward into the depths of ether and there seized and sped onward by the impulsion of the light. Is the electric field strong enough for this action? Assuredly. An electric field of two hundred volts per meter would suffice. Such are familiar near the earth's surface, and far intenser ones must prevail in the regions of polar light.

Undoubtedly it is a most perilous voyage upon which such a germ of life sets out from the system say of α Centauri to that of our own sun. The immense majority of these ether-farers would almost certainly be lost and perish; but here and there some one would arrive after nine thousand years safe at the utmost borders of our solar world and there by chance light upon some grain of sundust in the reflection of the zodiacal light and be borne along therewith sunward within the planetary ring and even into the atmosphere of some planet, as our own, which would seize upon it with gravitation and slowly drag it to the earth. Perilous too would be the landing on the shores of Time, yet some lucky sailor would succeed and so would establish a form of life upon this planet. Surely a most tremendous conception, striking wonder and awe through the hardest heart! Amazing too it is to reflect that this prodigious idea, so carefully wrought out and bulwarked at every point by the adamantine pillars of mathematical calculation, should have been anticipated in its grandest proportions by the brooding fancy of the pre-Christian Gnostics! For in the Naassene scriptures preserved to us by the good bishop Hippolytus in his *Refutatio omnium haeresium* we find creation described allegorically in the parable of the Sower as the sowing down of seeds from the unportrayable Godhead. Moreover in the deep-thoughted Gnostic Basilides we find repeatedly the same idea, and the new knowledge has actually adopted his favorite technical term "Panspermia" as its own, to express this most recent of astronomico-biological ideas. Life then, at least in its germs, is everywhere pulsing and throbbing throughout the universe and when the finger of time points to the accepted moment the myriad forms of life leap into being out from the teeming womb of ether. We may also see that these forms are probably nearly the same, at least closely allied even at opposite poles of the Milky Way, and man may feel his blood kinship with the tenants of the remotest world. It is impossible then to repress the suggestion that the actual forms of life, being everywhere what they are, have in themselves some deep-lying hitherto unsuspected reason for being thus and not otherwise, some reason profound as the properties of numbers or the logical necessities of the geometry of Euclid.

May one not even venture to hint that the biological import of the corpuscular theory is not yet exhausted? That in the almost endless divisibility of the atoms into sub-atoms or electrons there inheres some undiscovered connection with the pangenesis required in theories of heredity? That the mysteries of Mendelism and Mutation may yet be illuminated by flashes of electric light as in a polar sky at midnight? But even were I able, there is no time to pursue this thought further.

After all, however, what do we know of electricity? Lord Kelvin declared that in his age he understood it no better than in his youth. The colossal theory we have been considering assumes all the fundamental and hitherto inexplicable laws of electric action. In particular, besides the ether and its wonderful properties, it assumes the law of inverse squares as the mode of all interaction of electrons. Herein of course it is perfectly justified, but the mind cannot lay the importunate query, Why does this interaction vary precisely as the inverse square of the distance? Moreover, since all this action takes place in the uniform universal ether whose regions are distinguishable only by the motions that affect them, the mind is led irresistibly to the conjecture that the problem is ultimately hydrodynamical, that all these electric phenomena must some way be thinkable as movements in an all-pervading medium. Hence then the great significance of the vortex theory developed by Thomson (Kelvin) from the central property of vortex filaments discovered by Helmholtz. Kelvin and his disciples thought to recognize the indissoluble atom in the indissoluble vortex-ring and imagined that an exhaustive doctrine of knots would yield a list of atoms or elements. The new knowledge shows indeed that the atom is not indissoluble, nay, is in some cases actually dissolving, but the vortex-ring or filament is not thereby deprived of its scientific importance. It may yet be that the sub-atom or electron is essentially a vortex in ether, and the theoretic properties of such vortices may be the observed properties of electrons. At this point then the question arises: What is the relation between the equations of electricity and the equations of hydrodynamics? Or between the electromagnetic and the hydrodynamic fields? Or, finally, between the interactions of electric units and of hydrodynamic elements? It seems hard then to exaggerate the moment attaching to such researches as those of the two Bjerknes (father and son) upon fields of force, researches both experimental and mathematical.

They have proved that the relation in question is certainly a close one, that nearly all the elementary actions assumed in electromagnetic theory may be surprisingly simulated by actual experiments on pulses in a liquid medium. True, there presents itself a queer paradox: the hydrodynamic field appears not as the direct but as the inverted image of the electro-magnetic field, attraction and repulsion in the one answering to repulsion and attraction in the other. But in spite of this perversion, the results remain highly interesting and point the way along which research must follow till something more satisfying shall be suggested or discovered.

Meantime it is no more important to see clearly the wide range and immense perspective of the new knowledge than it is to recognize unequivocally its necessary limitations. Even when we suppose the hydrodynamic analogy perfect, even were it possible to state all the facts of the material and ethereal worlds, in a word, of the world of space and sense, in terms of motion rotational or irrotational in a universal ether of whatever properties, even though the vision of the Laplacian Intelligence should thus be actualized on a scale far grander than Laplace himself ever dreamed of, it would still remain true that the real problem of the world was just as far as ever from solution. For let it be understood once for all that this problem is not even to be stated finally in terms of mass and motion, the sole concepts available in the corpuscular or any other physical theory. Mass and motion are not ultimates in human thinking, no physical concepts can be ultimates. The supreme all-comprehending fact of the world is Mind, Soul, Spirit, and the ultimates of all thinking, of all reality, must be psychical. The physical world is an idea, a sensible form, which the mind constructs at every instant by the inherent law of its own activity. It is a splendid, a glorious construct, a real construct, well worthy the everlasting study and admiration of its own creator. Contemplating this amazing creation the Spirit beholds its own image as in a mirror, and it may even explore the depths of its own being by interpreting backward its own image; just as the mathematician may translate the analytic (algebraic) properties of his equation into geometric properties of the corresponding locus and again may interpret the geometric properties of this locus into corresponding algebraic properties of the original equation. But the equation is not the locus, nor ever can be, nor would it cease to be, nor change its properties, if the geometric construction were quite impossible. Speaking then in allegory one may declare that the psychical world is a sublime equation of infinite degree; the physical world is its majestic geometric locus, its construct in terms of time and space, mass and motion. Between the two there subsists or may subsist some one-to-one relation. Let us study the grand image with unreserved admiration and with unflagging zeal. But let us never forget that after all it is only an image, a stupendous parable. Let us never forget the great word of Goethe: *Alles Vergängliche ist nur ein Gleichniss.* Otherwise the brightest achievements of physical and physiological research may prove to be only traps for our unwary feet. Otherwise we shall surely fall into the pit of materialism; we shall mistake the significant for the significate, we shall see in the whole universe only an interplay of corpuscles, and shall talk with Cabanis of the brain secreting thought as the liver secretes bile. Such an issue would be deplorable beyond all words, it would indeed be a bankruptcy of science absolutely hopeless. As secondaries electrons are invaluable; as primaries they are absolutely worthless. The favorite maxim of Sir William Hamilton abides in full validity:

> "On earth is nothing great but man;
> In man is nothing great but mind."

But if even the sublimest flights of physical speculation vouchsafe us no glimpse beyond the veil, how shall we ever lift it? What gaze of reason shall ever penetrate its folds? Who knows indeed that in the nature of the case it can be lifted? For my part I should be content to think it a veil of Isis. Is there not a kind of inspiration in the thought of revelation after revelation, forever and ever, world without end?

> "Higher than your arrows fly,
> Deeper than your plummets fall,
> Is the deepest, the Most High,
> Is the All in All."

Chase without catch would indeed be disheartening, but chase with nothing more to catch would certainly be empty and uninviting. If the chase is to be eternal and yet inspiring, the quarry must be infinite, the supreme truth must be forever approachable but never attainable! In the ancient myth it is said that Egeus concealed from Theseus the secret of his birth, burying the evidence beneath a stone by the seaside. The Father of heaven and earth has perhaps secreted somewhere the proofs of the origin and nature of all things; but upon the shore of what ocean, O man of science, has He rolled the stone that hides them?[3]

WILLIAM BENJAMIN SMITH.

NEW ORLEANS, LOUISIANA.

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[3] Adapted from Maurice Guérin's *Le Centaure*.

PURPOSE AS SYSTEMATIC UNITY.

I.

THE present investigation is undertaken for the sake of the light which it may throw on the problem of value. Assuming that value is a function of what may broadly be termed "interest," it becomes imperative to get at the fundamental or generic character of this phenomenon. What is that attitude or act or process which is characteristic of living things, which is unmistakably present in the motor-affective consciousness of man, and which shades away through instinct to the doubtful borderland of tropism? Both the vocabulary and the grammatical structure of language provide for the teleological categories. "Purpose," "means and end," "in order to," "for the sake of," "with a view to"—these and many other kindred forms of speech are evidently applicable to the same context. There is something in our world to which they serve to call attention. What is it?

I propose to view the matter objectively rather than introspectively. What we wish to discover is the nature of the thing, and not the nature of the consciousness of the thing. It is fair, I think, to apply the analogy of mechanism. One would not think of approaching this latter question by an examination of the consciousness of mechanism. Similarly, purpose is supposed to be a kind of happening or chain of events differing in its determination from that of mechanism. It may appear that consciousness is incidental to the purposive kind of determination. But in that case we should begin with the process as a whole and work in. We should not shut ourselves up in advance to the view that purpose takes place only in the introspective stream of consciousness. We cannot, in other words, determine the role of consciousness in purpose unless we first take that view of the matter in which both consciousness and its physical context are taken into account. Behavior or conduct, broadly surveyed in all the dimensions that experience affords, can alone give us the proper perspective. We want if possible to discover what it is to *be* interested, not what it is merely to *feel* interested. What is implied in *being* favorably or unfavorably disposed to anything? It may be that it all comes to nothing more than a peculiar quality or arrangement among the data of introspection, and in that case a structural psychology of feeling, will, desire or ideation will tell the whole story. But such a conclusion would be equivalent to an abandonment of the widespread notion that purposiveness is a kind of determination of events differing from their mechanical determination. The really important claim made in behalf of purpose is the claim that things happen *because* of purpose. Are acts performed *on account* of ends? Is it proper to *explain* what takes place in human or animal life, or in the course of nature at large, by the categories of teleology? The most exhaustive introspective analysis of the motor-affective consciousness would leave this question unanswered, and to confine ourselves to the data which such analysis affords would be to prejudge it unfavorably.

It is, of course, permissible to suppose that even though a case should be made for purpose in a physical or cosmic sense, value should be limited to subjective purpose. But it is evident that the question would then be merely one of terms. If there are objective purposes as well as subjective, it would be necessary to employ some term to designate the objects of both attitudes. It would be important to construe subjective purpose as a species of this broader genus, which would be accomplished best by taking it as a kind of valuing. Furthermore objective or dynamic purpose, if there be such, would be far too important in its bearing on the special value-sciences to warrant our disregarding it in a general theory of value.

II. NEGATIVE MEANINGS.

In looking for a clue to the meaning of dynamic or objective interest, we must free our minds so far as possible from the purely negative associations which the teleological terms have acquired. The case for teleology is prejudiced by a suggestion of anti-scientific bias, or of unscientific laxity. This is due no doubt mainly to the religious or popular auspices under which it has been advanced. The teleological hypothesis is often invoked to satisfy aspirations, to flatter human nature or to conceal ignorance. In the present controversy over vitalism the proofs of purposiveness seem to consist mainly in the indictment of mechanism. Purpose is not recommended on account of its own success, but on account of the failure of something else. When so invoked it means little more than that unknown factor, x, which is needed to complete the explanation of such phenomena as growth or organic equilibrium. It is not surprising that vitalists should be regarded as impatient scientists who cannot wait for a rigorous experimental solution, but must needs invent an agency *ad hoc*; or at best as irresponsible critics who remind plodding science of its outstanding difficulties without assisting in the serious work of overcoming them.

The party of teleology according to this view is a sort of opposition party to the real scientists, who are sobered by being in power. It is the function of this opposition party to challenge and censure, rather than to legislate and administer. It can afford to be careless or premature because it is not in office. For itself it has no policy, but confines itself to impeaching those policies of mechanism, determinism, naturalism and experimentalism which authoritative science is patiently executing. There is doubtless a certain merit in this two-party system in science. But certainly for our constructive purposes there can be no virtue in a conception of purpose as merely not something else. If the conception is to be of any use to us it must have a positive explanatory value of its own.

Nor is it at all necessary to suppose that purpose is the contradictory alternative to some other hypothesis such as mechanism. Purpose must doubtless be *different* from mechanism if it is not to lose its identity altogether; but that it should be incompatible with mechanism, or observed only in its breach, does not follow. Through being thus regarded from the outset as an antithesis to an established and universally credited theory, teleology needlessly makes enemies for itself. There is certainly no reason to suppose in advance that teleology is less compatible with mechanism than statics with dynamics, or the atomic theory with the electro-magnetic theory of light. There is certainly an empirical presumption to the contrary. A man who goes to his journey's end *in order to* keep an engagement, does not appear to violate the law of gravitation in so doing. Let us therefore endeavor to get the positive sense of the teleological type of explanation, and let us say of its compatibility or incompatibility with the mechanical type of explanation, that that is as it may be.

III. PROVISIONAL DEFINITION.

We start with the popular supposition that there is a peculiar and specific mode of explanation, which may certainly be employed in the case of the rational conduct of man, which may probably be applied to lower forms of life, which may for speculative reasons be extended to the cosmos as a whole, and for which the name is "purpose."

1. Let us first consider a case of *human conduct.* An off-hand provisional view of this alleged mode of explanation is afforded by Socrates's famous allusion to Anaxagoras in Plato's "Phædo." Socrates, it will be remembered, distinguishes two ways of explaining his being in prison. On the one hand it is to be explained by reference to his bones and muscles. But this, he thinks, would be an inappropriate explanation; not untrue to be sure, since bones and muscles do supply the necessary "conditions,"—but not the sort of explanation that touches the real cause of a *mind*'s acting. The second and preferred explanation is in terms of Socrates's purpose of "enduring any punishment which the law inflicts." A mind, in other words, acts for the best, according to its lights. To explain its action, therefore, it is necessary to discover what it deems best, and then to construe the particular act as an instance of that best. In the present case it is supposed that Socrates is actuated by the general principle of submission to the law, and that he has judged his remaining in prison to be what under the existing circumstances that principle implies.

Let us analyze the situation more carefully. lest we omit any essential factor. In the first place, there must be a general type of action, such as submission to law, of which a particular act, such as remaining in prison, may be regarded as an instance. In the second place, there must be an agent possessed of a stable disposition or tendency to perform acts of a certain class, under varying circumstances. The particular performances will differ according to circumstances, but they must be consistent in some respect. Then, thirdly, there must be some determinate relation between the rule or type of action and the agent's disposition. But what is this determinate relation? The simplest alternative is to suppose that the rule of action is identical with the constant or consistent feature of the disposition. Thus we might suppose that Socrates tended under varying circumstances to submit to the law. But this will not do. For if it should happen that his remaining in prison were as a matter of fact not what the law required; if it should happen that there had been some error in transmitting the commands of the authorities, or if it should turn out upon reflection that Socrates's escape rather than his passively yielding to tyrannical oppression was more in keeping with his constitutional rights, that would not disprove his purpose to submit to law. What is necessary is that Socrates should *mean* to submit to law, or that he should *think* his act to be a case of submitting to the law. The link between the rule and the disposition is an act of interpretation or judgment. In other words, one is said to be governed by a purpose M, when M is some generalized form of action, and when one is disposed consistently to perform what one believes (whether correctly or mistaken) to be a case of M.

2. This, then, appears to be what is meant by purpose when purposiveness is imputed to the rational or reflective procedure of man. Let us now turn to what common sense would regard as a more doubtful case of purpose, the case of *animal behavior*. The differentia of animal behavior which was first remarked, was the power of self-motion. Whereas an inanimate object merely submitted to motion imparted to it from without by impact, a living thing seemed to be an original source of motion. Associated with this phenomenon was the relatively unpredictable character of the action of living organisms. What they did was so far due to internal and unobservable factors that you could not rely on their yielding in any uniform way to the operation of the external forces that you might observe or apply. Living things had a way of moving of

themselves, without any apparent cause which might serve to put you on your guard. Hence they were said to exhibit "spontaneity."

It is still customary to characterize living things in this way. Biologists describe the organism as "an active, self-assertive, living creature—to some extent master of its fate."[1] But this spontaneity or self-motion no longer serves to distinguish living from inanimate things, owing to the development of the science of energy. We should now speak of this apparent spontaneity as a release of stored energy. The organism accumulates chemical energy by the process of metabolism, and then discharges it when subjected to some kind of stimulation from without. When the discharge occurs it is out of all proportion to the stimulation. Indeed in some cases there appears to be no external stimulus at all. In any case the internal factor is so much more important than the external factor that the latter affords no safe basis for prediction. As organisms become more elaborate the discharge comes to depend more and more upon the quantity and balance of its stored energies and less and less upon what is done to it from without. But even so this phenomenon of release or discharge does not differ in principle from what happens in the case of combustion or in the case of the action of high explosives. If the behavior of living things is spontaneous in this sense, there is also "spontaneous combustion" in the same sense. In the one case as in the other we now suppose that the action would be predictable even with the utmost quantitative precision if we knew the internal organization of the acting body, as well as the character and intensity of the stimulus. It is merely a question of the relative preponderance of central over peripheral factors.

Hence we are at present inclined to look elsewhere for the differentia of life, and to find it, not in the spontaneity of action, but in its direction *toward* something. The explosion, we say, is blind and aimless,—indifferent to consequences; whereas life is circumspect and prophetic. Forewarned is forearmed. This is what we mean when we speak of living things as exhibiting intelligence. We do not credit all living things with intelligence; but we have no hesitation in imputing it to the higher forms of animal life, and the phenomena of instinct and tropism have led to our imputing at least a quasi-intelligence to the lower animals and even to plants.

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[1] Thomson, *Heredity*, p. 172.

In so far as we impute intelligence to living things, we feel the need of explaining their action in a peculiar way. The explosion is satisfactorily accounted for as a resultant of two physically existing factors, the internal organization of stored energies and the external spark or trigger. But in the case of intelligence it seems necessary or at least appropriate to refer to the sequel, to that which is merely in prospect at the moment when the action occurs. Thus a dog moves rapidly away, or gets behind some intervening obstacle, when his master takes down the whip. In so far as this implies intelligence we think of it not in terms merely of existing chemical energy and the light impinging on the optic nerve. We take account also of what is going to happen, namely the painful beating. We say that that also explains why the animal is acting as he does. Or we say that the animal is acting "in order to avoid" the beating. But since the beating which is avoided does not as a matter of fact occur, we are thus appealing to a factor which is in some sense merely possible or hypothetical. Over and above the animal's power of spontaneous motion, over and above the external action of the stimulus, there is some additional factor which refers to this mere possibility and which decisively determines the direction which the discharge takes. I do not mean to assert that this third factor cannot be traced to the previous experiences of the animal. Probably it can; and this has led comparative psychologists to associate intelligence with docility, or the capacity to "learn by experience." But that is not the point. However he may have come by it, the animal is supposed at the moment of action to possess a capacity for prospectively determined action. He acts not because of what is or has been merely, but because of what *may be* by virtue of his action, or what *would be* without his action. He acts, we say, from fear of a painful whipping, or from hope of immunity. There is no way of describing either the fear or the hope, without admitting it to be the fear or the hope *of something*, which something is not upon the plane of past or present physical existence as ordinarily conceived.

If, now, we put together the results of the analysis of our two examples we shall have a provisional view of interested or purposive action. In both cases there is an organism with certain accumulated energies and certain organized propensities. In both cases there is a specific external situation which acts upon the organism and liberates the energies and propensities. So far there is nothing to distinguish these cases from such physico-chemical analogies as I have cited. But in both cases there is a third and differential factor which constitutes their purposive aspect. The act is construed by the agent in terms of something ulterior and non-existential. Socrates judges his act to be of the general type of submission to law; to the dog the whip is a sign of beating or pain-to-come, and his flight is a response "as to" pain. In both cases the agent views the situation whether by inference or association, in the light of some aspect or relation that transcends given fact; and his acting as he does is determined by his viewing it as he does.

Jennings has termed this characteristic of behavior "reaction to representative stimuli." "The sea urchin. . . .

responds to a sudden shadow falling upon it by pointing its spines in the direction from which the shadow comes. This action is defensive, serving to protect it from enemies that in approaching may have cast the shadow. The reaction is produced by the shadow, but it *refers,* in its biological value, to something behind the shadow.

"In all these cases the reaction to the change cannot be considered due to any direct injurious or beneficial effect of the actual change itself. The actual change merely *represents* a possible change behind it, which *is* injurious or beneficial. The organism reacts as if to something else than the change actually occurring. The change has the function of a *sign*. We may appropriately call stimuli of this sort *representative* stimuli."[2]

The same general principle applies to the higher organism, Socrates. That which releases his action is a representation. His friends come to his prison and urge him to escape. Their actions and words are a sign to him of law-breaking and *as such* he resists them; or his presence in prison represents to him submission to law, and as representing that, he holds to it. Let us now refine this notion of interest or purpose by comparing it with other notions which approximate it, but in some respect fall short of it or depart from it.

IV. THE PATHETIC FALLACY.

The most familiar error regarding purpose is the so-called "pathetic fallacy." It will be worth our while to inquire just wherein the fallacy lies. Suppose that in spite of my most painstaking efforts to execute a powerful stroke, the golf ball rolls ingloriously from the tee. I then turn and rend my new driver or call down maledictions upon it. I am angry not with myself but with it. I feel resentment toward it precisely as though it had meant to spite me. I virtually attribute malice to it. Now this, as my less heated partner may remind me, is unreasonable, because the golf stick really didn't mean it or do it "on purpose." It is true that in effect the stick thwarts me. The stick is a cause of my displeasure. But the error consists in imputing that displeasure to it as a motive or ground for its action. In other words, it is not sufficient for purposive action that its effect should occasion displeasure; it is necessary that this displeasure as a prospective contingency should determine the act. Or take another example. Basking in its warmth, I praise the sun and feel gratefully disposed to it. If I knew what the sun liked I would gladly reciprocate. This is an innocent error, a kind of poetic license, but error it is none the less. For I have responded to the sun as though the pleasure which its rays were about to give me had actuated the sun in shedding them; whereas this effect upon my sensibilities is accidental and in no way needed in order to account for the radiation of the sun's light and heat.

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[2] H. S. Jennings, *Behavior of the Lower Organisms*, p. 297.

But there is also a positive implication in this criticism. My own action in each case *is* purposive. My addressing the ball, or lying in the sun, is to be accounted for by reference to the stroke or the bodily comfort that is to come. My error lies not in employing such a mode of explanation but in misapplying it. There is a human weakness, doubtless one of the major motives in religion, which prompts one to extend to all the agencies involved in any event that purposive type of determination which really holds only of one's own participation in it. In the case of one's own agency the prospective sequel does account for the act, but in the case of the other contributory agencies this explanation is out of place; or, some but not all antecedent agencies are determined by the sequel. Not to discriminate is to commit the inverse of a common fallacy.

It would not be inappropriate to term this characteristic teleological error the fallacy of "*ante hoc ergo propter hoc.*"

There is a further point which this error brings to light. In so far as I like it the sun's warming my body is good. The effect of the sun's action is therefore good; and it might even be that the sun "tended" to warm my body and so to do good. But that is evidently not sufficient to make the sun's action purposive. Action resulting in, or tending to, good is not *ipso facto* purposive action. It would be purposive only provided that result were somehow *accountable* for the action. In other words we are forced to recognize the essentially dynamic character of purpose. It is not the quality of the results, whether good, bad or indifferent, that implies the purposiveness of its antecedents, but the function of that result as somehow participating in the *determination* of the process.

V. PURPOSE AND SYSTEMATIC UNITY.

1. Among the widely accepted notions of purpose or interest which we shall find it profitable to examine, the next is that which identifies purpose with *systematic unity.* This notion is distinguished by the fact that it disregards the time factor, or regards it as accidental. Purpose of this sort may characterize the world *sub specie eternitatis.* It may qualify a static whole, and appear in its mere structure or arrangement, regardless of its origin or history. It follows that the purposiveness of any given reality may be judged by internal evidence, even when it is supposed that the reality in question was produced by conscious design. A purposive object is believed, like Paley's watch, to exhibit its "designedness" in its very form. This formal, static purposiveness is identified with order, system,—the interrelation of parts in a whole. Let us first consider examples, beginning with an example in which the time factor is clearly eliminated.

An ellipse is more than a mere collection of individual points; it is a curve having a distinctive character as a whole, which may be expressed by the equation $x + y = c$. Every individual point in the curve is a value of the variables in this equation, and its position is determined according to the law by the position of the other points. Although the position of each point differs from that of every other point, there is at the same time a certain identical character among them all, namely the "$x + y$" character, or the sum of the distances from two fixed points called the "foci." To call this a unified whole means that there is a definite whole-character in terms of which all of the constituents can be described. This whole-character is the law of the parts, prescribes their positions, or, as it is sometimes expressed, "generates" them. In the case of a broken line or a curve having no equation, there is no whole-character except the merely collective aspect of the several points. In that case the parts are prior to the whole, and to speak of them as parts of the whole is therefore circular or redundant. But in the case of the ellipse the whole is prior to the parts, or comes first in the order of explanation. The parts, therefore, are said to be governed by something ulterior to them. The ellipse does not exist except in so far as all the points are in their proper positions, and yet their being so disposed is determined by the nature of the ellipse. The ellipse is then said to be the purpose which regulates the several points. Each point is determined by what is necessary in order that there shall be an ellipse.

Let us now turn to examples in which time figures as one of the internal factors of a unified whole. The whole is not in time, but time is in the whole. First let us take an example of what is commonly regarded as mechanism. Suppose a body to be moving in a straight line at a uniform velocity, governed by the law of inertia. Although each successive position of the body is new, a certain ratio of its distance-interval and its time-interval measured from any previous position is always the same. Its kinematic history as a whole exhibits a definite character which prescribes what its position must be at each particular moment. It may in its actual behavior be construed as a realization of the principle of uniform velocity. This principle in itself is a universal or ideal entity. It does not exist except in and through the successive positions of a moving body which obeys it. And yet these positions are themselves somehow determined by it.

Let us take one more example, one that is less precise but is drawn from the context of life. Modern civilization may be said to possess a characteristic flavor, which distinguishes it as a form of life. It is conditioned by the co-presence and cooperation of a thousand factors, such as the present phase of geological evolution, temperate climate, fertility of soil, racial blend, cultural tradition etc. But these many factors *compose* something. There is a unique and simple quality which somehow supervenes when all these factors are aggregated,—a quality which is identical with none of them and yet somehow takes them all up into itself. In terms of this one quality we can construe all the various conditions as contributing this or that to it. Through it they become, not so many miscellaneous particulars, but various aspects or phases of one thing. This resultant quality, or *Gestaltsqualität*, is their purpose by reference to which they are now seen to be *for* something. They may now be understood not merely severally but collectively. There is a reason why they should be together; or, over and above that determination which accounts for each by itself, there is a determination which accounts for each in its relation to the others. But this determination springs somehow from a character which does not come into existence until after they are all in place.

These examples serve to give plausibility to the notion that is now before us. Let us analyze them more carefully. It will be found, I believe, that the notion of unity which they illustrate is divisible into two types, which I shall call "ideal" and "existential" unity. The first is based on the conception of a universal. A universal unifies its instances. Furthermore it has this peculiar relation to any instance of itself: it explains the instance, or serves as a description of it, and in that sense appears to be prior to it; but on the other hand it exists, or is exemplified only through the instance, and in that sense appears to be posterior to it. So that a case of a universal seems to be something that *is* only through itself. Interrelation is an example of ideal unity. When a number of terms possess a mutual relation exclusively, that is, when they are related among themselves as none of them is related to any term without, they compose a whole. Or they may all sustain a common relation to a term outside the group. Or they may be instances of the same set of universals where the universals are themselves interrelated.

The second, or existential, type of unity consists of the convergence or fusion of many existences into one. The unity lies not in any universal or set of universals under which many particulars may be subsumed, but in an ulterior particular. Whereas unity of the first type is intelligible or apprehended by reason, unity of this second type is sensible or is a matter of empirical fact. The several particulars work together to produce a singular result, or blend into an individuality that is directly felt. Let us inquire, then, whether either of these conceptions of unity, that of universality or that of individuality, will serve as a definition of purpose.

2. It is to be noted at the outset that purpose would be an all-pervasive feature of the world we live in. Instead of its being the exception it would be the rule. Instead of

its being a residual aspect of the world, complementary to that aspect with which the physical sciences have to do, it would coincide with that orderly and law-abiding aspect of nature of which physical science has been the principal exponent. Instead of its being the antithesis to mechanism, mechanism would itself supply the most perfect instances of it. This will doubtless serve to recommend it in the judgment of those who have a predilection for teleological monism. But such philosophers cannot escape the price of their easy speculative victory. For in so far as a conception is universal it is relatively colorless. To characterize the world as purposive in this general formal sense is to say nothing more than every scientist or materialist asserts. It does not differ from saying that it is determined and intelligible in terms of laws. Democritus and Spinoza would then be as good teleologists as Plato or Leibniz. And quite apart from its philosophical barrenness such a view would be wholly inept for the purpose of a theory of value. It would wholly disregard the peculiar or differential feature of those phenomena which biology, economics, ethics and esthetics study, and would be of no service whatever in distinguishing and coordinating these sciences. Although this pragmatic objection might be thought to justify our dismissing it, it will be instructive to discover if possible just wherein this view fails to agree with our provisional conception.

3. Unity may be thought to *constitute* purpose, or to imply a purposive origin. In other words the purpose in question may be thought of as internal to the system, or as external. When intelligible or ideal unity is thought of as itself constituting purposiveness it is evident that the common view from which the teleological terms get their initial meaning, is virtually abandoned. Consider first the simple relation of a universal to its instance. A certain given curve is, let us say, an ellipse. The universal ellipse gives

the curve its character, or serves as a description of it; while on the other hand the curve gives existence or embodiment to the general nature ellipse. There is no paradox here provided we distinguish the sort of status which a universal enjoys from the status of existence. There is a peculiar relation between a universal and its instance whereby the first qualifies the second and the second realizes the first. Now it means nothing to say that the curve exists *in order to* realize the ellipse. It simply *does* realize the ellipse. The ideal nature of the ellipse explains *what* the curve is; but it does not explain the fact *that* the curve exists. Compare the case of Socrates cited above. The purposiveness of Socrates's act lay not in the fact that it was an instance of submission to law, but in the fact that its being such in some sense accounted for its occurrence. We express this by saying that Socrates performed the act because he deemed it such. In other words, the particular case of being submissive to law which in fact ensued was a condition of its own occurrence, through being referred to as a hypothetical possibility by the mind of Socrates. To construe the curve similarly it would be necessary to impute to the curve as determining its existence some reference to the possibility of its being an ellipse; which would imply a complexity of determination for which there is here no justification.

In the case of existential unity or individuality, it is admitted that a variety does possess a unitary aspect, but it cannot be said that any term of the manifold exists *for the sake of* that unity. The peculiar flavor which supervenes upon an assemblage of historical conditions is not necessarily accountable for any of them. It is not necessary to suppose that the conditions were in any sense determined by their composing a unity. This would be the case only provided among the determining factors of each condition there were one which referred to the composite sequel;

which might, of course, be the case, but could not be argued merely from the fact of the supervening unity.

The situation is not altered if we suppose any degree or any combination of these types of systematic unity. If nature throughout observes the law of gravitation, or that of the conservation of energy, so that every bodily event is an instance of the same set of interrelated universals—if it be possible to describe everything in nature by one formula, this would not in the least imply that nature exists for the sake of realizing the formula. If the world as a whole should possess a simple flavor or quality to which every existence and every event contributed an indispensable condition, this would not in the least imply that such a cosmic *quale* determined its conditions. In short mere unity as such, whether it be a conceptual unity or a perceptual unity, does not constitute purpose. This does not prove that purpose does not involve unity, but only that its differentia must lie in something else.

4. But it may still be supposed that unity argues an *external agency* of a purposive sort, that unity is a *product* of purpose. In the first place, it is to be observed that unity furnishes an almost irresistible opportunity for the pathetic fallacy. There is a strong human interest in unity, an intellectual and practical interest in ideal unity, and an esthetic interest in existential unity. When nature is found to obey relatively simple laws, and so to be predictable and workable, the mind rejoices and praises God. When sky and sea and land compose a pleasing landscape, or when a thousand different conditions conspire to bring about the existence of fuel or food, one feels instinctively grateful. And so strong is the instinct that it creates its own object. But we may dismiss this impulse as an amiable weakness. We have already seen that the fact that a state of things is an object of interest, is no proof that that state of things is owing to interest.

A second argument for the purposive origin of unity is the argument from analogy, the argument that Paley employed in the case of the watch. A thing of the type which man makes on purpose is presumably made on purpose, if not by man then by God. There is a curious paradox connected with this argument. Man is peculiarly addicted to making machines, or things which work uniformly and automatically. That being the case those parts of nature which argue a purposive creation ought to be those parts which are most mechanical, such as the periodic motions of the stars, or the conservation of energy. A living organism differs from the typical human artefact just in so far as it is spontaneous and unpredictable; and ought therefore to be the last thing to be attributed to a creative will. As a matter of fact, however, the mechanical parts of nature are the originals of which human artefacts are adaptations and imitations. Machines are made after the analogy of nature, and their machine-like character is due to what they borrow from its independent and self-sufficient forces. Invention does, it is true, correlate these forces in new ways; but there is nothing in the principle of correlation that is new. One could not look for a prettier correlation of forces than that between the centrifugal and centripetal forces of a planet moving in an elliptical orbit. The fact is that man can contrive for his own ends physical systems which resemble those which he finds in nature. The remarkable or unaccountable thing is not that systematic unity should appear in the absence of purpose, but that purpose should have anything to do with it at all. The original mechanisms of nature are relatively intelligible, and human artefacts relatively doubtful and obscure. Purposive origination is not to be invoked as a helpful hypothesis to account for a mystery; it is itself the mystery which the mechanical laws of nature will presumably help to solve.

If the argument from analogy is to be employed at all, there is more justification for arguing from the case of nature to that of human conduct than for arguing in the reverse direction. If the hypothesis of purpose is needed at all, it is needed to explain not the existence of systematic unity in the world, but the peculiar case of human conduct or animal behavior.

Nor is the case for the argument from analogy strengthened if the emphasis is put on the aspect of utility. A systematic unity which serves human needs does not require an explanation which refers to these needs. The periodic motions of the earth evidently provide the heat and light and intervals of rest without which human life would be impossible. Their utility exceeds that of any man-made agency. But to suppose that they have come about for the sake of this, is simply to lapse into that pathetic fallacy which we have already dismissed.

5. There is one further argument from unity which deserves consideration, the argument, namely which employs the notion of *probability*. It is argued that in proportion as a coincidence is *remarkable* it must have been designed. Thus, for example, Professor Henderson has shown that the physico-chemical constitution of the natural world is uniquely favorable to life. It constitutes a maximum of fitness.

"The fitness of the environment results from characteristics which constitute a series of maxima — unique or nearly unique properties of water, carbonic acid, the compounds of carbon, hydrogen, and oxygen and the ocean—so numerous, so varied, so nearly complete among all things which are concerned in the problem that together they form certainly the greatest possible fitness. No other environment consisting of primary constituents made up of other known elements, or lacking water and carbonic acid, could possess a like number of fit characteristics or such highly fit characteristics, or in any manner such great fitness to promote complexity, durability, and active metabolism in the organic mechanism which we call life."[3]

The author then goes on to argue that "there is not one chance in millions of millions" that all these properties should simultaneously occur, and that they should be thus uniquely favorable to life, unless we assume some general law that determines them so to be.

Now, in the first place, this appears to be a misuse of the principle of probability. It is not proper to infer a law from a single simultaneity, but only from a succession of simultaneities. If the first throw of a pair of dice happens to be a double-six, that does not prove that the dice are loaded, in spite of the fact that the chances were thirty-six to one against that particular combination. There would be ground for suspecting a partiality for double-sixes only provided in the long run this combination turned up more frequently than once in thirty-six times. The general or original physico-chemical composition of the cosmos is like a single throw of dice; the chances are heavily against it, but this proves nothing as to any determining principle over and above chance. It would be possible to make such an inference only provided it were possible to gather in the cosmic elements and throw them again. It makes no difference whatever how heavy the odds are against any particular combination, provided there is only one instance of the combination; for it is entirely in keeping with a combination's unusual or remarkable character that it should occur first. In other words, the principle of chance has to do with the frequency of a combination and not with its place in the series. Where the range of alternatives is large the first combination will always be highly improbable; but this fact follows from the principle of chance, and cannot create a presumption against chance.[4]

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[3] L. J. Henderson, *The Fitness of the Environment*, p. 272.

The same reasoning holds of the "fitness" of the environment for life. Let us suppose life to be a constant. It will then be comparable to a die having the same number on all of its faces. The environment, on the other hand, has millions of faces only one of which matches the first die. That the two should match in any single instance is highly improbable; the chances are millions to one against it. But if it should happen that there was only one trial, its happening to be successful would prove nothing as to there being anything more than chance at work. Professor Henderson insists that the relation of fitness between life and its environment is reciprocal; but he appears to ignore this essential fact, that it is the environment which is given once and for all, while the die of life is thrown again and again. It may be argued that life agrees with its environment too often to permit one to suppose that on the part of life it is a matter of chance. But nothing of the sort can be inferred on the part of the cosmic environment because that lies unchanged upon the board. The relation of matching where one term is cast once and the other repeatedly is not a reciprocal relation. If the matching is uniformly successful, it may prove that the matcher is not trusting to chance, but it proves nothing as to the matched.

Suppose that we vary the illustration. It is a remarkable fact that a given individual likes the world just as he finds it. The world agrees with his taste. In view of the vast range of possibilities, the countless worlds that would offend him, this is prodigiously improbable. But it does not follow that the world is determined to please him.

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[4] Bosanquet makes this clear when he says: "We have very small ground for being surprised at the actual occurrence of that alternative which had fewest chances in its favor; and absolutely none for being surprised at the occurrence of a marked or interesting alternative which has against it enormous odds." (*Logic*, second edition, Vol. I, p. 342.)

That would follow only provided the world came up again and again according to his taste. But, unfortunately for the argument, the world does not come up again and again, but only once. Suppose, on the other hand, that sentient beings come up again and again always liking the given world. This, then, *would* argue that the taste of sentient creatures was somehow determined with reference to their environment, and did not originate independently of it.

Even this would not prove purpose. Suppose all the impressions on a given area of sand to correspond exactly and uniquely to the feet of a certain child that is at play in the neighborhood. This would presumably not be an accident; but would be accepted as evidence that one of the terms of the fitness relation, namely the feet of the child, was the cause of the other, namely the impressions on the sand. It would be necessary, however, to distinguish this case from the relation between the same child's feet and the shoes in his closet. There is fitness in both cases; and in both cases the fitness is determined, not accidental. But in the latter case alone would one say that the fitness was due to purpose. One would not argue the purposiveness from the bare relation of fitness, or from the non-accidental character of the fitness, but from the peculiar way in which the fitness was in this case determined. The shoes in the closet are of a certain shape because of being judged or expected to fit their owner. And this might still be the case even though they should as a matter of fact fit very poorly.

6. We conclude, then, that purpose in the provisional sense adopted at the outset, cannot be said to consist in the structural unity of any system taken as a whole; nor can it be inferred from such a unity, as necessary to account for its uniqueness, maximal character, aptness or any other peculiarity. The same condition of unity might or might not have been due to purpose. It is necessary in each case

to observe the actual course of its coming into existence. In other words, purpose is not to be defined in general formal terms, any more than chemical reaction. It is not the same thing as determinateness or law in general. If there be such a thing, it consists in a particular sort of agency that appears in some cases of determination and not in others. We dissent, then, from the view that purpose is exhibited in all cases of system and unity; being exhibited most unmistakably in those realms of nature that science has already set in order, and more doubtfully, therefore, in the phenomena of life.[5] We agree with those who find purpose to be a peculiarity attaching to some parts of the existent world, most unmistakably to the behavior of man; purpose in the inorganic world being a doubtful extension of a conception derived from the datum of life.

RALPH BARTON PERRY.

HARVARD UNIVERSITY.

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[5] I understand that this latter is the view to which "objective" idealists incline, as illustrated by the case of Bosanquet. Cf. his "Meaning of Teleology," *Proceedings of the British Academy*, Vol. II: "The foundations of teleology in the universe are far too deeply laid to be accounted for by, still less restricted to, the intervention of finite consciousness. Everything goes to show that such consciousness should not be regarded as the source of teleology, but as itself a manifestation, falling within wider manifestations, of the immanent individuality of the real. It is not teleological because, as a finite subject of desire and volition, it is 'purposive.' It is what we call 'purposive' because reality is individual and teleological, and manifests this character partly in finite intelligence, partly in appearances of a far greater range and scope" (pp. 8-9). This "individuality of the real" which manifests itself in the larger cosmic and historical processes, where we cannot suppose it to be designed or commanded by any finite mind, would appear to consist in systematic unity of the sorts which we have defined.

THE ORIGIN OF TAOISM.[*]

THE western world is apt to regard Chinese reflection as predominantly ethical. This is due largely to the fact that the system of Confucius is taken as typical.[1] But this view is misleading and requires to be supplemented. In reality the Chinese mind is fundamentally concerned for the health of the inner man, and accordingly it is more properly described as ethico-spiritual. This appears to the careful reader in the teachings of Confucius himself, and it is notably true of the mystical doctrine of Lao-tze and his more immediate followers.

Taoism is well named after the central principle (Tao) which pervades this system of thought. The original meaning of the term was "way" (path), which in the realm of moral inquiry came to mean "norm of conduct"; in time it was narrowed to mean "the rational principle in man," and then later it was extended to signify "reason in man and reality." This transformation was brought to definite accomplishment by the real founder of the system, as I believe, Lao-tze (sixth century B. C.), who was concerned to find a metaphysical basis for his ethico-spiritual convictions and to that end hypostatized the principle of Tao. Thus a convenient analogue in western thought is Reason or Logos mystically conceived.[2]

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[*] Partial publication (Part I, revised and abridged) of thesis entitled: "The Thought of Lao-tze; its origin, content and development," presented to Northwestern University in partial fulfilment of the requirements for the attainment of the degree of Doctor of Philosophy. The whole will appear in book form in the publications of the Open Court Publishing Company.

[1] The common view is well seen in Grube when he says ("Die chinesische Philosophie," in *Kultur der Gegenwart*, I, v, p. 66, 2d ed., Berlin 1913) that "überhaupt das Chinesentum in Konfuzius seine vollendetste und ausgeprägteste Verkörperung gefunden hat.... Will man die chinesische Kultur mit einem kurzen Schlagwort charakterisieren, so wird man sie als konfuzianisch bezeichnen." This is very misleading. Confucianism came to be dominant over Taoism in China partly because of the royal edict of Wu-Ti (139-85 B.C.), which exalted this thought at the expense of all other, and partly because of the universal difficulty of popularizing mysticism or adapting it to institutional life. But while Confucius has had more visible effect in China the effect of Lao-tze has been more profound. "It is not Confucianism so much as Taoism which has most profoundly influenced the Chinese mind." This statement by Chang-Tai-Yen, a noted scholar and my former revered teacher, I believe gives the real truth of the matter, and it should be carried in mind always in studying Chinese thought.

Concerning the life of the founder we know very little in detail, and of his work we have only the Tao-Teh-king which tradition attributed to him.[3] Both the historicity of Lao-tze and the authenticity of his work have been questioned. But it is my belief that, in the existing state of our data, these doubts have been disposed of definitively by Carus.[4] Certainly the proper procedure here is first to attempt to justify the tradition before rejecting it because of difficulties in the Tao-Teh-King. The determining factor in this connection must be a real understanding of that work. If it can be viewed as a unitary whole produced by a single mind, the tradition may be taken as confirmed beyond question. In considering its content systematically I will hope to show that this can be done. For the present my concern is to indicate how the thought of Lao-tze can be considered in the historical continuity of Chinese reflection, after the manner of the western treatment of the history of philosophy. To that end we must deal with it as a product of preceding thought and immediate environment and the genius of our author.

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[2] The term "Tao" of course long antedates the time of Lao-tze. As early as the Shu-King its ambiguity is already evident, where it means "way" (*wan-tao,* or "royal way," as the norm to which all should conform) and also "rational part of man" (*tao-sin,* or rational heart, as distinguished from *jhren-sin,* or human heart). Herein lay the germ for the development from the moral to the definitely metaphysical. The transition was therefore from "way" to "right way of life," to "life according to reason," to life in accordance with the rational principle of all reality, including man." It was this last idea which was elaborated by Lao-tze in a world-view.

[3] The Tao-Teh-King is accessible to the English reader in the excellent translation by Carus (Chicago, 1898; [rev. ed. 1913]) where (pp. 95, 96) the brief account of Lao-tze's life, by Sze-Ma-Chien, may also be found in English translation. This account gives his place of birth, family, official connection (custodian of the royal archives and state historian) and relates an encounter with Confucius. "He practised reason and virtue" we are told, and that his teaching was directed to "self-concealment and namelessness." When he foresaw the decline of his state he left for the frontier, where the custom-house officer urged him to write a book before leaving his country. "Thereupon," concludes the account, "he wrote a book of two parts consisting of five thousand and odd words, in which he discussed the concepts of reason and virtue. Then he departed. No one knows where he died." The term Tao-Teh-King was not employed before the second century A. D., but the sayings which constitute this work were uniformly referred to Lao-tze as their author. It had been customary to name books after the writer.

[4] See his admirably judicious article, "The Authenticity of the Tao-Teh-King," in *The Monist,* Vol. XI, 1901, pp. 574-601. It is my belief that the western reader of Chinese literature is in danger of hasty conclusions from the difficulty of understanding the Chinese way of thinking. The Chinese mind

The rise of a new viewpoint in the development of thought cannot be an entirely isolated affair, however novel the addition may be. This may be safely assumed for the progress of Chinese thought as it is for that of the west. Hence one may properly expect that a system such as that of Lao-tze's in the Tao-Teh-King could not have appeared without a preceding development and that accordingly it should be studied in its historical setting.

The earliest Chinese reflection centered in the conduct of man and is embodied in the Hong-Fan, which dates back to 2205-2198 B. C. and forms a part of the Shu-King (the oldest book of China). Therein we find rules laid down for

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does not move normally in the channels of discursive reasoning because it is essentially intuitionistic. Insight rather than dialectic engages their attention. Hence the westerner may too readily suspect forgery in what appears to be nonsense (cf. La Couperie, *Western Origin of Chinese Thought*, p. 124, where he shrewdly observes this). The cautious reader will bear in mind the conciseness of diction in the Tao-Teh-King as standing for thought far deeper than appears, and also that the circumstances of writing precluded fuller elaboration, as well as the inevitable errors of copyists where the thought of the text is obscure in itself. In particular it is important to pay due regard to the purity of style and soberness of thought which signalize the Tao-Teh-King in contrast with the later works of the school. A stream cannot rise higher than its source, and a forgery would necessarily have revealed those fantasies and vagaries which are so conspicuous in the writings of the later Taoists. The unsympathetic reader is apt to be robbed of insight both for seeing this obvious fact and also for getting the real meaning at the heart of the perversions and aberrations.

the ordering of one's inner life, the securing of proper balance between conflicting tendencies in one's nature, the relation that subsists between man and the natural world-order as well as that between man and his fellows. In it we find too the conception of the king as the embodiment of the eternal moral principles, the "royal way" (*wan-tao*) which was conceived of as the objective criterion to which men should conform their personal preferences. And in it we find the notion of Tao also as "rational part of man," as above indicated. The idea of Tao therefore goes very far back in Chinese thought.

In addition to the Shu-King there is also the Yih-King, or "Book of Changes."[5] Therein is outlined the first Chinese cosmological scheme, as well as an ethical doctrine based on this cosmology. It posits an original principle called *Tai-Chi,* the "Great Origin," and two primary forces called *Yin* and *Yang*. It was thought that the world was formed through the action and reaction between these two principles. A cosmos was regarded as possible only when there was a perfect balance between these two basic elements, otherwise chaos would ensue. The attendant ethical doctrine centered in the notion of moderation. As in the objective order so in man an equilibrium of opposite forces was the aim. Going to extremes was regarded as disastrous, because contrary to the course of nature. The cosmology and the ethics of the Yih-King were therefore constituent elements in Chinese reflection long before they appeared in the Tao-Teh-King.

In addition to these two sources there were probably other documents which were later lost, as the quotations in the Tao-Teh-King would indicate. Moreover, the accounts of the lives of ascetics make plain that from early times there had been men who lived in seclusion, insulated from the currents of social and political life. With the advent of the period of storm and stress, at Lao-tze's time, this ascetic spirit became much intensified. It took deep hold on the thoughtful and serious-minded men of that age, some of whom betook themselves to rural pursuits while others moved about apparently without profession, eccentric and mysterious in behavior.

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[5] The rudiments of this work were in existence prior to the date of the Shu-King, but were not elaborated until about 1200 B. C.

In the Tao-Teh-King the connection with the past is evidenced by certain expressions[6] which indicate clearly a consciousness of debt to preceding thought. This has long been recognized by Chinese scholars and has been largely responsible for the impulse to find the origin of Taoism in reflection antecedent to Lao-tze. Thus Hwang-ti, the legendary emperor of the Chinese, has been regarded as the founder of Taoism, though on very meagre evidence.[7] Again it has been suggested that Lao-tze was simply the transmitter of wise sayings and proverbs out of the past.[8] Another account makes Lao-tze to have sat under a master, Shan Yung, who was already advanced in years.[9] Still another view finds the origin of Taoism in the Yih-King, whose cosmology and ethics bear so striking a resemblance to those of the Tao-Teh-King.[10] In short, Chinese scholars have been amply aware of a continuity between preceding reflection and that of Lao-tze, and the connection is so obvious that there is danger of thereby overlooking his originality.[11]

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[6] Such, for example, as "The Ancients say," "The Poet says," "The Sage says" and the like.

[7] Based on the fact that a passage of the Tao-Teh-King is quoted from a book attributed to Hwang-ti no longer extant. The same passage is found at the beginning of the work of Lieh-tze. The existence of such a book was denied by Hwai-Nan-tze.

[8] By Chu-Hsi (1130-1200 A. D.)

[9] See Hwai-Nan-tze (ch. 10) Lao-tze "learned the lesson of tenderness by watching the tongue." The allusion is to old age when the teeth have fallen out.

[10] See Yih-King, especially Books III, VI and XI, Engl. transl. by Legge (*Sacred Books of the East,* Vol. XVI).

[11] Cf. Carus, *op. cit.,* p. 31, and Strauss, *Lao-tze's Tao-Teh-King,* pp. lxiii ff.

That Lao-tze had free and full access to the literature of his day is sufficiently attested by the tradition which made him custodian of the royal archives and state historian. This included the classical literature which has survived and probably much that has since been lost.[12] It is inconceivable that a contact of this kind should have failed to influence the development of his thought. In addition there were certain records of the hermits or recluses who preceded him and to whose general circle he is supposed to have belonged. The contempt for temporal goods, the effort to create a world of their own beyond that of ordinary values, the spirit of thoroughgoing renunciation which characterized this group are essentially the marks of the thought of Lao-tze. Such influence of his predecessors and contemporaries in thought must therefore be assumed if we are to avoid the impossible idea that the construction of Lao-tze was wholly *de novo*.[13]

Thus it is clear that Lao-tze enjoyed the intellectual heritage of his age. But we must recall that this heritage reveals no such systematic character as may be found in the Tao-Teh-King. This work is so characterized by simplicity and unity, it so bears the impress of a single individual, that it suggests inevitably to the reader who has entered into its spirit a seamless fabric woven from the deeply experienced convictions of a distinct personality. One must therefore assume some genius operative in revitalizing and bringing *en rapport* with his age the inherited creased more and more until the emperor became a mere figurehead, a negligible factor, and the real power passed into the hands of the vassals. With this came a contest among the various states for supremacy, and so the nation was precipitated into a tumultuous maelstrom of strife. The balance between the forces which make law and order possible had become violently disturbed. Factional strife and internecine feuds became the order of the day. There ensued a reckless rush for self-aggrandizement and an unscrupulous disregard of rights, and brute power replaced reason. To supplement the military force the resources of craft and cunning were pressed into service and the Machiavellian attitude became dominant.

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[12] The Shi-King, Yih-King and Lih-King would have been accessible to Lao-tze in their ancient form and not as revised by Confucius.

[13] The possibility of foreign influence in the shaping of Lao-tze's thought, either direct or indirect, I do not consider here. Where the effort is made (e. g., by Harlez, Douglas, La Couperie, Strauss, Rémusat, in varying degrees) the proof rests upon mere resemblance in mystical or mythological or religious conceptions. Such procedure is too open to the charge of precipitate generalization on the basis of fancied resemblance and too hazardous in the absence of supporting external evidence to win more than doubtful assent. It may be true that such foreign influence did exist in fact. But the state of historical knowledge is at present entirely inadequate to furnish satisfactory conclusions. It seems therefore to me more desirable to seek to account for Lao-tze by reference to indigenous conditions.

Along with the political decline went hand in hand a cultural deterioration. In place of the earlier devotion to peaceful pursuits, with its cultivation of arts and literature, there arose an exaggerated emphasis upon material values, and the earlier simplicity was supplanted by sophistication both in thought and in action. In this rule of unreason the complex social organization, which the first few rulers had succeeded in building up, had completely collapsed. At the beginning of the dynasty, especially in the reign of Chen-Wang (1115-1079 B. C.), there had been worked out an elaborate system of etiquette, which in point of complexity has no parallel in history.[17] But in these troublous times this fell to pieces. Neither the weaklings on the throne nor the contending vassals were inclined to maintain this elaborate system. And where all forces were working for disintegration naturally all phases of the social life were affected. The established ethical standards also broke down to be superseded by personal whim and caprice. Nowhere could universal rules of conduct be found, as in the ancient days. Unjust laws were enacted in place of the old regulations, which had been so nicely calculated to promote orderly life. The life of the people was made miserable by all sorts of oppressive measures, and their very life-blood was drained that the craving of the rulers for military glory and the excitement of the chase might be satisfied. In short, a condition of affairs existed which was strikingly similar to that which prevailed in France prior to the Revolution. Wherever one looks he is confronted with unreason and disorder resulting from the chase after worldly gain and the abuse of power.

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[17] In its ramifications it extended to every phase of social and political life. Regulations were prescribed even for such details as mode of dress, eating, toilet, form of address, etc., etc. Its apparently immutable and fixed character testifies to the genius for organization of its author, Cheo-King, and also accounts for the fascination which it exercised over the mind of Confucius later who felt impelled to refer to that period as the great age of culture.

Such were the conditions prevailing in the world into which both Lao-tze and Confucius were born. The intensity of the crisis may be measured by the fact that China's two greatest creative thinkers arose at this time, after whom really significant thought in that country continued to develop. The system of each was adapted to solve from its angle the problem set by the aggravated situation. Confucius was conservative and sought to reconstruct in harmony with the past, while Lao-tze was radical and could be satisfied with nothing short of complete breach. Each may be conceived as crystallizing the spirit and thought of the type which he represented. The temperament of the one was essentially institutional and accordingly gave itself to reconstructing the social fabric as existing, as is abundantly clear out of all his writings. The temperament of the other was wholly impatient with all temporal expedients and would not stop short of permanent peace in some eternal principle; this he found by reconstructing the ancient Tao as supreme principle of men and reality, as also amply appears in his work, the Tao-Teh-King.

The contrast between the two men was really antipodal"[18] and by reference to it the signficance of the genius of our author stands out at its highest. Confucius was characterized by moderation and sanity as the world of common sense measures these qualities. In his efforts at reform he confined himself wholly to the attainable, in conformity with the sagacity of the plain man. His keen sense for concrete reality forbade him to step forth with anything like a Utopian program. He clung to the solid ground, with never a desire to soar in the empyrean realms. He was no doctrinaire, no mere theorist in any sense, but a practical reformer. To mend the situation as he saw it he set about to abolish the feudal system, as the source of disintegration, and to reestablish the monarchy with its stabilizing force of imperial power. To counteract the forces that were making against law and order he set out to revive the doctrines of the ancient sages, the system of Cheo-li, whose exact and rigid orderliness very naturally fascinated his type of mind. Hence his supreme emphasis on ritual and his belief that the golden age lay in the past.

But the spirit of Lao-tze was radically different and permitted no such direction as that of Confucius in his solution of the problem. His genius impelled him to make a clean sweep and led him to a very different reconstruction. He felt that the world had gone so far astray that it could not be reformed by mere revival of ancient traditions or by any other patching-up process. He demanded some radical procedure, a complete reversal of the existing order. He felt deeply the insecurity, nay, the utter collapse of the foundations of life in his age, and he sought a basis so secure that it might not be shaken. Like Plato, so much in this his fellow-spirit of the Occident a century and a half later, he regarded the present order as wholly bad and not to be compromised with. And like Plato he turned away from the immediate world of strife to the life of reflection and contemplation, to find a world that was characterized by the eternal as opposed to the temporal. But more mystical than Plato he found his solution by way of the inner life and communing with nature. In revolting against the existing order he was driven to withdraw from externals like the true mystic that he was. And in so withdrawing he found within his inner self the supreme principle of his own and of all being. Thus he was enabled to give new life and meaning to the doctrine of Tao, as a simple and unitary principle of all reality.

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[18] This contrast is revealed in beautiful simplicity in the report by Sze-Ma-Chien concerning the interview between the two men (Carus, *Tao-Teh-King*, pp. 95, 96). The difference between these men is vividly portrayed by Grube, who writes: "Auf der einen Seite ein Mann, der mit beiden Füssen auf dem Boden der Wirklichkeit steht und....nur nach dem Erreichbaren strebt. Auf der anderen Seite das Wolkenkuckucksheim eines einsamen, weltfremden Denkers. Dort zielbewusstes Streben nach staatlicher Reform auf sittlicher Grundlage, hier asketische Weltflucht und mystisches Versenken ins ewige Tao."

To this abiding principle he called his wayward people to return. In opposition to the spirit of self-assertion that pervaded the age, he called for complete renunciation, for the surrender of the petty ambitions of the ego which only in this way could realize Tao. Instead of the feverish and scattered haste so common in his day, he enjoined quiet confidence in the fundamental reason of the universal order. Against over-regulation and the multiplication of laws and statutes he therefore went the full length of a doctrine of *laissez-faire*. He would have none of the ceremonies and rules of etiquette on which the conciliating Confucius later laid such stress; they were for him the most prolific source of the great evil of hypocrisy, being merely external show. All parading of virtue or even conscious well-doing was for him an evil. He would eliminate all virtue except that of acting according to Tao and all knowledge save that of Tao. This was the sum and substance of his thought. And the solution which he disclosed to his age as the way of salvation was an unfolding of this.

But Lao-tze did not stand alone in this negative attitude toward the existing order of things. He was a true spokesman for those fellow spirits of his race and day who had also turned unreservedly to the inner life for refuge from the storm of the external world. Like all great leaders of thought, our philosopher gave form and body to the longings and aspirations in the minds of the many less gifted. He is clearly the concentrated embodiment of the quietistic and mystical spirit of the recluses already referred to. They were in need of a spokesman to make clearly articulate what they felt and experienced, and this was supplied by Lao-tze. As the genius of Confucius enabled him to serve as a constructive guide for the type he represented, so the genius of Lao-tze enabled him to create for and direct the less numerous but relatively widespread number of the opposite type.[19]

Such then was the place of Lao-tze in the origin of Taoism. He was its real founder because it was his genius that established it. What had grown up during long centuries and undergone gradual transformation was brought by him to articulate formulation under the impulse of an environment which pressed to a mystical solution. His fundamental doctrine was the long familiar Tao, but its central position and multiple unfolding in man and in reality required the labor of genius for establishment. Lao-tze was that genius, and so Chinese history has recorded him as one of its two great creative thinkers. Accordingly his doctrine, as set down in the Tao-Teh-King, is found to exhibit the unity and simplicity which signalize that work. It is essentially the reaction to a most difficult situation of a born mystic who was able to give full expression to the mysticism of his people. And what has been said of the mystic in general maintains for Lao-tze in an eminent degree. "What the world, which truly knows *nothing,* calls 'mysticism,' is the science of *ultimates,*. . . . .the science of self-evident reality, which cannot be 'reasoned about,' because it is the object of pure reason or perception."[20] Herein is contained the key to the true understanding of Lao-tze's work.

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[19] In the Confucian Analects alone reference is made to fourteen such recluses who ridiculed the effort to reform a decadent society. The fortuitous character of these meetings and the fact that they are recorded by Confucius and his disciples attest how widespread the movement was. Strauss (*op. cit.*, pp. xliii ff) has suggested the ingenious theory that there was already in existence a Taoist sect (*Tao-Gemeinde*), whose teachings were reduced to writing by Lao-tze. There is no basis in fact for this conjecture, and it overlooks the real ability of Lao-tze. But this is undoubtedly a more correct direction for interpretation than that which disregards the widespread nature of the movement.

In this connection it is of great importance to bear in mind, contrary to a too prevalent misconception, that even Confucius had to give up his efforts at reform in despair in his later years, and that he was forced to content himself with the more quiet work of teaching and of editing books. The real significance of his work lay in this preparation for posterity rather than in his actual effect on his own age.

KING SHU LIU.

NANKING UNIVERSITY.

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[20] Quoted from Patmore by Underhill (*Mysticism,* 4th ed., 1912, p. 29).

THE CONTRIBUTIONS OF PARACELSUS TO MEDICAL SCIENCE AND PRACTICE.

THERE appears to be little doubt as to the real value of many specific contributions of Paracelsus to medical knowledge and practice, although competent authorities differ widely as to the extent and character of his influence upon medical progress. It may be admitted that his vigorous assaults upon the degenerate Galenism of his day were effective in arousing an attitude of criticism and questioning which assisted greatly the influence of other workers whose labors were laying less sensationally but more soundly the foundation stones of scientific medicine.

Vesalius, often called the founder of the modern science of anatomy, and Paré, the "father of surgery," were both contemporaries of Paracelsus, though their great works appeared only after the death of Paracelsus. The "Greater Surgery" of Paracelsus had appeared nearly thirty years before Paré's classical work and had passed through several editions, and it is said that Paré acknowledged his indebtedness to Paracelsus in the preface to the first edition of his work.[1]

Admitting that none of the medical treatises of Paracelsus has the scientific value of the works of his great contemporaries, it should nevertheless not be forgotten that his work may have had an influence for progress in his own time much greater than its present value in the light of later knowledge. Dr. Sudhoff records some nineteen editions of the "Greater Surgery" by the close of the sixteenth century, in German, French, Latin and Dutch languages, and other works of his shared in somewhat less degree in this popularity.

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[1] Cf. Stoddart, *The Life of Paracelsus.* London, 1911, p. 65.

The disapproval and hostility of the universities and the profession toward Paracelsus should not be permitted to mislead us into underrating his influence, as it may be recalled that both Vesalius and Paré also suffered from this hostility. Vesalius was denounced by his former teacher Sylvius as an insane heretic and his great work on anatomy was denounced to the Inquisition. Though he was not condemned by that body his professorship at Padua became untenable, and he was forced to return to his native city Brussels and is said to have become a hypochondriac as the result of his persecutions.

Paré was more successful in maintaining his professional position through official support though the faculty of the University of Paris protested his tenure of office.

The history of medical science and discovery has been the subject of more thorough study than most of the natural sciences, and a number of competent critics of early medical history have estimated the place of Paracelsus in the development of various departments of that science. From such sources may be best summarized the contributions of Paracelsus.

Thus with respect to surgery, Dr. Edmund Owen in the *Encyclopaedia Britannica* (eleventh edition, article "Surgery") says:

"The fourteenth and fifteenth centuries are almost entirely without interest for surgical history. The dead level of tradition is broken first by two men of originality and genius, Paracelsus (1493-1541) and Paré, and by the revival of anatomy at the hands of Andreas Vesalius (1514-64) and Gabriel Fallopius (1523-1562), professors at Padua. Apart from the mystical form in which much of his teaching was cast Paracelsus has great merits as a reformer of surgical practice. It is not, however, as an innovator in operative surgery, but rather as a direct observer of natural processes that Paracelsus is distinguished. His description of hospital gangrene, for example, is perfectly true to nature; his numerous observations on syphilis are also sound and sensible; and he was the first to point out the connection between cretinism of the offspring and goitre of the parents."

So also Proksch,[2] the historian of syphilitic diseases, credits Paracelsus with the recognition of the inherited character of this disease and states that there are indeed but few and subordinate regulations in modern syphilis-therapy which Paracelsus has not enunciated. Iwan Bloch also attributes the first observation of the hereditary character of that disease to Paracelsus.[3] That Paracelsus devoted so much attention to the consideration of these diseases was evidently made a subject of contemptuous criticism by his opponents as may be inferred from his replies to them in the *Paragranum*.[4]

"Why then do you clowns (*Gugelfritzen*) abuse my writings, which you can in no way refute other than by saying that I know nothing to write about but of *luxus* and *venere*? Is that a trifling thing? or in your opinion to be despised? Because I have understood that all open wounds may be converted into the French disease (i. e., syphilis), which is the worst disease in the whole world,—no worse has ever been known,—which spares nobody and attacks the highest personages the most severely—shall I therefore be despised? Because I bring help to princes, lords and peasants and relate the errors that I have found, and because this has resulted in good and high reputation for me, you would throw me down into the mire and not spare the sick. For it is they and not I whom you would cast into the gutter."

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[2] Quoted by Baas, *Geschichtliche Entwickelung des ärztlichen Standes*, p. 210.

[3] Neuburger und Pagel. *Handbuch der Geschichte der Medizin*, III, 403.

[4] Paracelsus, *Opera*, Strassburg Folio, 1616. I, 201-2.

Dr. Bauer[5] calls attention to the rational protest of Paracelsus against the excessive blood-letting in vogue at the time, his objections being based on the hypothesis that the process disturbed the harmony of the system, and upon the argument that the blood could not be purified by merely lessening its quantity.

"For the healing art and for pharmacology in connection therewith," says Dr. E. Schaer in his monograph on the history of pharmacology,[6] reform is in the first instance attached to the name of Theophrastus Paracelsus whose much contested importance for the rebirth of medicine in the period of the Reformation has been in recent times finally established in a favorable direction by a master work of critical investigation of sources. . . . But however much overzealous adherents of the brilliant physician may have misunderstood him and have gone at times beyond the goal he established, nevertheless the historical consideration of pharmacology will not hesitate to yield to Paracelsus the merit of the effective repression of the medieval polypharmacy often as meaningless as it was superstitious and to credit him with having effectively called attention to the pharmacological value of many metallic preparations and analogous chemical remedies."

Dr. Max Neuburger[7] thus summarizes the claims of Paracelsus to a place in the history of the useful advances in medicine:

[5] *Geschichte der Aderlässe,* 1870, p. 147.

[6] Neuburger and Pagel, II, 565-6.

[7] Neuburger and Pagel, II, 36ff.

"Under the banner of utilitarianism Paracelsus rendered the practical art of healing so many services that in this respect his preeminent historical importance cannot be doubted. In bringing chemistry to a higher plane and in making the new accessory branch useful to medicine, in comprehending the value of dietetics, in teaching the use of a great number of mineral substances (iron, lead, copper, antimony, mercury), and on the other hand in teaching the knowledge of their injurious actions; in paving the way to the scientific investigation of mineral waters (determination of the iron contents by nut galls), in essentially improving pharmacy (with his disciples Oswald Croll and Valerius Cordus) by the preparation of tinctures and alcoholic extracts. . . .he has achieved really fundamental merit for all time."

It was also no unimportant service that Paracelsus rendered to medical science in attributing to natural rather than to the mystical influence of devils or spirits such nervous maladies as St. Vitus' dance. It is doubtful perhaps if his influence in this direction was very immediate upon contemporary thought, at least if we may judge from the sad history of the trials, tortures and executions of witches during a century after the activity of Paracelsus.

Doubtless also the fantastic character of the philosophy of Paracelsus itself served to diminish the effect of his sounder and saner thought.

A distinguished student of the history of science, Andrew D. White, thus characterizes the services of Paracelsus in this direction.[8]

"Yet in the beginning of the sixteenth century cases of 'possession' on a large scale began to be brought within the scope of medical science, and the man who led in this evolution of medical science was Paracelsus. He it was who first bade modern Europe think for a moment upon the idea that these diseases are inflicted neither by saints nor demons, and that the 'dancing possession' is simply a form of disease of which the cure may be effected by proper remedies and regimen. Paracelsus appears to have escaped any serious interference; it took some time, perhaps, for the theological leaders to understand that he had 'let a new idea loose upon the planet,' but they soon understood it and their course was simple. For about fifty years the new idea was well kept under, but in 1563 another physician, John Wier of Cleves, revived it at much risk to his position and reputation."

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[8] *History of Warfare of Science and Theology*, II, 139.

An interesting thesis maintained by Paracelsus was the doctrine that every disease must have its remedy. The scholastic authorities had pronounced certain diseases as incurable, and they were accordingly so considered by the profession. Rejecting as he did the ancient authorities, Paracelsus naturally enough rejected this dogma as necessarily true. Manifestly also he believed that he himself had with his new remedies effected cures of certain of these diseases, though he makes no pretension to be able to cure all diseases. The history of medical thought and discussion shows that this thesis of Paracelsus was a frequent subject of partizan debate during the century after Paracelsus.

Paracelsus sustains his thesis, however, not by the method of modern science—upon evidence of experiment and observation—but by the philosophical or rather metaphysical argument of its *a priori* reasonableness in the divine purpose, and by his interpretation of the doctrines of Christ.

"Know therefore that medicine is so to be trusted in relation to health—that it is possible for it to heal every natural disease, for whenever God has entertained anger and not mercy, there is always provided for every disease a medicine for its cure. For God does not desire us to die but to live, and to live long, that in this life we may bear sorrow and remorse for our sins so that we may repent of them."[9]

"There is yet another great error which has strongly influenced me to write this book,—namely, because they say that diseases which I include in this book are incurable. Behold, now, their great folly: How can a physician say that a disease is incurable when death is not present; those only are incurable in which death is present. Thus they assert of gout, of epilepsy. O you foolish heads, who has authorized you to speak, because you know nothing and can accomplish nothing? Why do you not consider the saying of Christ, where he says that the sick have need of a physician? Are those not sick whom you abandon? I think so. If then they are sick as proven, then they need the physician. If then they need the physician, why do you say they cannot be helped? They need the physician that they may be helped by him. Why then do you say that they are not to be helped? You say it because you are born from the labyrinth [of errors] of medicine, and Ignorance is your mother. Every disease has its medicine. For, it is God's will that he be manifested in marvelous ways to the sick."[10]

This is obviously setting dogma against dogma, and opposing to scholasticism the methods of scholasticism. Yet that this dictum of Paracelsus was not without influence upon contemporary thought is evidenced by a passage in the writings of Robert Boyle in the century following.[11]

"Though we cannot but disapprove the vainglorious boasts of Paracelsus himself and some of his followers, who for all that lived no longer than other men, yet I think mankind owes something to the chymists for having put some men in hope of doing greater cures than have been formerly aspired to or even thought possible and thereby engage them to make trials and attempts in order thereto. For not only before men were awakened and excited by the many promises and some great cures of Arnaldus de Villanova, Paracelsus, Rulandus, Severinus, and Helmont, many physicians were wont to be too forward to pronounce men troubled with such and such diseases as incurable and rather detract from nature and art than confess that these two could do what ordinary physick could not, but even now, I fear, there are but too many who though they will not openly affirm that such and such diseases are absolutely incurable, yet if a particular patient troubled with them is presented, they will be very apt to undervalue (at least) if not deride those who shall attempt to cure them."

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[9] Paracelsus, *Liber de religione perpetua*. Sudhoff, *Versuch einer Kritik*, etc., II, 415.

[10] Par., *Op.* I, 253. "Die erste Defension."

[11] Boyle's Works, Birch's ed., I, 481.

His rational consideration and treatment of wounds and open sores is worthy of note. Instead of the customary treatment of closing up by sewing or plastering, or covering them with poultices and applications, he advocated cleanliness, protection from dirt and "external enemies," and regulation of diet, trusting to nature to effect the cure. "Every wound heals itself if it is only kept clean."[12]

There is no doubt that Paracelsus enjoyed a considerable reputation as a skilful and successful practitioner, and there is contemporary testimony, as well as his own statements, to show that he was frequently sent for even from long distances to treat wealthy and prominent patients whose maladies had baffled the skill of the Galenic physicians.

It is of course true that popular reputations of physicians are not always the true measure of ability even in our day. Nevertheless there seems little reason to doubt in spite of the assertions of hostile critics of his time, that with his new remedies, his keen observation, and his unusually open mind, he was indeed able to afford relief or to effect cures where the orthodox physicians trammeled by their infallible dogmas were unsuccessful. That his new methods sometimes did harm rather than good is quite possible. That would naturally be the result of breaking radically new paths. And an independent empiricism—a practice founded upon experiment and personal observation seems to have been his practicce and his teaching, "*Experentia ist Scientia.*" It seems probable that in his dealings with the sick, his fantastic natural philosophy was rather subordinated to a native common sense and practical logic. As stated by Professor Neuburger (*op. cit.*, II, 35), "We see in Paracelsus....the most prominent incorporation of that enigmatic, intuitive, anticipative intelligence of the people, which drawing upon the unfathomable sources of a rather intuitive than consciously recognized experience, not infrequently puts to shame the dialectically involved reasoning of scholasticism."

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[12] Cf. Helfreich in Neuburger and Pagel, III, p. 15.

Paracelsus has indeed clearly expressed his opinion that theories should not be permitted to dominate the practice of the physician.

"For in experiments neither theories nor other arguments are applicable, but they are to be considered as their own expressions. Therefore we admonish every one who reads these, not to oppose the methods of experiment but according as its own power permits to follow it out without prejudice. For every experiment is like a weapon which must be used according to its peculiar power, as a spear to thrust, a club to strike,—so also is it with experiments.... To use experiments requires an experienced man who is sure of his thrust and stroke that he may use and direct it according to its fashion."[13]

That he endeavored to keep an open mind toward the symptoms of his patients, not too much governed by preconceived dogmas, is also indicated in his defense against certain attacks of his opponents in which they accuse him of not at once recognizing symptoms and treatment:

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[13] *Chir. Bücher*, Fol. 1618, pp. 300-301.

"They complain of me that when I come to a patient, I do not know instantly what the matter is with him, but that I need time to find out. It is indeed true that they pronounce judgment immediately; their folly is to blame for that, for in the end their first judgment is false, and from day to day as time passes they know less what the trouble is and hence betake themselves to lying, while I from day to day endeavor to arrive at the truth. For obscure diseases cannot be at once recognized as colors are. With colors we can see what is black, green, blue etc. If however there were a curtain in front of them we could not recognize them.... What the eyes can see can be judged quickly, but what is hidden from the eyes—it is vain to grasp as if it were visible. Take, for instance the miner; be he as able, experienced and skilful as may be, when he sees for the first time an ore, he cannot know what it contains, what it will yield, nor how it is to be treated, roasted, fused, ignited or burned. He must first run tests and trials and see whither these lead....Thus it is with obscure and serious diseases, that so hasty judgments cannot be made though the humoral physicians do this."[14]

Admitting the value of the positive contributions of Paracelsus to medical knowledge and practice, the net value of the reform campaign which he instituted is variously estimated by historians of medicine. For it must be remembered that Paracelsus fought against dogmas intrenched in tradition, by dogmas of his own. To the fantastic theories of the Greek-Arabian authorities he opposed many equally fantastic theories. That by his assault upon the absurdities and weaknesses of the Galenic medicine of his time he paved the way for greater hospitality to new and progressive ideas is unquestionable, but that by this assault he also did much to discredit the valuable elements as well as the corruptions of ancient medical achievements is also true. It is very difficult to balance justly the progressive and the reactionary influences he exerted upon the progress of medicine, and naturally, therefore, authorities differ upon this question. Thus Neuburger (*op. cit.*) appreciates the value of the accomplishments of Paracelsus, yet doubts that he is to be considered as a reformer of medicine in the sense that was Vesalius or Paré, that is, he laid no foundation stones of importance and the real value of much of his thought required the later developments of modern scientific thought for its interpretation. His aim was to found medicine upon physiological and biological foundation but the method he chose was not the right method, and his analogical reasons and fantastic philosophy of macrocosm and microcosm were not convincing and led nowhere. The disaffection and discontent with conditions in medicine produced by his campaign, can, thinks Neuburger, hardly be called a revolution. That was to come later through the constructive work of more scientific methods.

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[14] *Op. fol.*, I, 262. (Die siebente Defension.)

In a similar vein Haeser (*op. cit.*) remarks "Scarcely ever has a physician seized the problem of his life with purer enthusiasm, served it with truer heart, or with greater earnestness kept in view the honor of his calling than the reformer of Einsiedeln. But the aim of his scientific endeavors was a mistaken one and no less mistaken was the method by which he sought to attain it."

A recent writer, Professor Hugo Magnus,[15] presents a more critical point of view:

"We must then summarize our judgment to this effect, that Paracelsus keenly felt the frightful corruption which medicine and the investigation of nature suffered from the hands of the Scholastics, but that he did not understand how to penetrate to the causes of this condition of his science. Instead of seeking in the scholastic system the root of this medical degeneration, he believed that it must be found exclusively in the healing art of the ancients. And thus he sought to shatter in blind hatred all that existed, without being in position to be able to replace the old theory he maligned by a new and better concept of nature and medicine. So Paracelsus wore away in unclear struggling, his bodily and mental energy, and lived indeed as a reformer,—a medical superman, in his own imagination, in his own valuation, but not in the recognition of his own times, nor in the judgment of posterity."

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[15] Hugo Magnus, *Paracelsus der Ueberarzt.* Breslau, 1906.

"If therefore I can find no relationship between the general methods of medicine to-day and the Theophrastic concept of nature, nevertheless our supercolleague must be considered in an essentially limited respect, to be sure, as the pioneer in certain modern points of view. He was the first to attempt the consideration of the phenomena of organic life in a chemical sense, and I do not need to emphasize that he thereby paved the way to a very powerful advance in our science. In this respect was Paracelsus a reformer, here he has pointed new paths in the valuation of pathologic phenomena as well as in therapy, even if here also he has theorized enough and allowed his neo-Platonism to play him many a trick."

By discarding and condemning all the ancient authorities, thinks Magnus, Paracelsus assailed not only the corrupted Galenism of his time but did much to discredit the positive achievements of the Greeks, and although the original Greek authorities were not the then prevailing texts, they were at least accessible in newly translated versions, and the good in them might have been incorporated and built upon by Paracelsus if he had possessed the scientific point of view. To the extent of his influence in this direction Paracelsus was therefore an opponent rather than a promoter of the progress of medical science. "Through his irrational theories he gave impulse to all sorts of mistaken notions among his followers, so that the wildest vagaries existed among the Paracelsists of the succeeding century."

The above will serve to illustrate the trend of modern critical judgment of Paracelsus as a reformer of medicine.

However estimates may vary as to the extent of the influence of Paracelsus as a reformer of medicine, credit must certainly be given him as a forceful agent in the downfall of the scholastic medical science of his time. The real reform in medical science, its establishment upon a basis of modern scientific method, was not the work of his century nor of the century to follow. Indeed it may not be too much to say that that great reform was mainly the work of the nineteenth century, and was made possible only through the patient labors of many investigators in the domains of physics, chemistry, anatomy, and biology.

If, however, we cannot claim for Paracelsus the unchallenged place of the reformer of medicine, we may at least recognize in him an earnest, powerful, and prophetic voice crying in the wilderness.

J. M. STILLMAN.
LELAND STANFORD JUNIOR UNIVERSITY.

THE ORIGIN OF THE MUTATION THEORY.

AT the time when Darwin published his book on the *Origin of Species* biological science was in a very different condition from what it is now. Hardly ten years had elapsed since Schleiden and Schwann discovered the fundamental law that all living organisms are built up of one or more ordinarily almost innumerable cells.

Mohl's contention that protoplasm is the essential and in fact the only living part of the cell is almost contemporaneous with Darwin's book (1849 and 1851). The presence of a nucleus within the cells began to be recognized. Hereditary problems were almost only discussed by breeders.

The *Textbook of Botany* by Julius Sachs appeared in 1868; it was the first to introduce into botany really scientific methods. When I was a student at the University of Leiden (1866-1870) systematic and descriptive morphological studies prevailed. Microscopical study of tissues was new and cytology had hardly reached us. Under these conditions a student interested in the causal relations of the phenomena of life naturally turned his mind to physics and chemistry. The prominent question of those days was the validity of physical and chemical laws in the living body. The idea dawned upon us that this question chiefly related to the protoplasm but hardly needed a proof for the cell walls and the tissues built up of them.

Once convinced that the phenomena of life are regulated by the protoplasm we naturally looked for methods of studying this relation. Many different ways presented themselves, and among these four seemed to me the most promising. They were the study of respiration, of galls, of osmosis and of variability. I tried all of them and at the end chose the last. Respiration was the source of energy; it was a phenomenon common to animals and plants, and one of the main links which connected both kingdoms in our knowledge at that time. I devoted many years to its study, chiefly in a comparative way, and chose it for the subject of my inaugural address when I was called to the chair of plant physiology in the University of Amsterdam (1878).

But galls seemed to promise far more. They are built up of the ordinary qualities of the plants combined in a new way to fit the requirements of their insects, and this combination is brought about under the influence of some stimulus given off by the insect. To discover the nature of these stimuli and the laws by which they so effectively change the growth of the tissues, seemed to me a scope worth the devotion of a whole life. I made a large collection of galls, in search of the species which would be the most appropriate to attack this line of research. I concluded for those of the willows, belonging to the genus Nematus. But at that period I met with Mr. M. W. Beyerinck who was far beyond me in the study of the life history of the galls, and so I left this pathway. I have, however, read a course upon galls and their bearing on the broad problems of biology about every third year from that time on.

The study of osmosis and of the turgidity of the cells led to the discovery of the semi-permeable membranes of the protoplasm and their significance for growth and movements as well as for the study of isotonic coefficients and the determination of atomic weights, as, e. g., in the case of the sugar raffinose. But its promise of elucidating hereditary questions diminished with every new discovery.

In 1880 I started a course on variability. I had been interested in this question chiefly by making a herbarium of monstrosities, and monstrosities were at that time almost all we knew of variability. Moreover I had visited the celebrated agriculturist W. A. Rimpau at Schlanstedt in Saxony and stayed repeatedly for some weeks on his estate in order to study his selection of cereals and sugarbeets. This induced me to take up a thorough study of agricultural and horticultural selection and I soon found that Darwin's books were the best guides for this literature. Especially from the pamphlets of Vilmorin, Verlot and Carrière I took a large part of the facts for elaboration of my lessons.

I read this course every second year from 1880 to 1900, and each time introduced into it the principles and methods which I found in the literature. This consisted partly in rare pamphlets which I succeeded in collecting only gradually, partly in articles scattered in agricultural and horticultural journals. In the meantime I increased my collection of monstrosities but soon perceived that collecting is not the right way to gain an insight into them. Therefore I preferred revisiting the same spots in nature for successive years and found the monstrosities regularly repeated. This induced the idea of their being heritable phenomena, a conception wholly new at that time, although the inheritance of the cockscomb or *Celosia* was, of course, known to every horticulturist. Then I turned to cultivation, made races of fasciated and twisted forms and studied the inheritance of pitchers and analogous deviations.

Parallel to these experimental studies I tried to penetrate into the theoretical side of the question, and this led to the publication of my book on *Intracellular Pangenesis* in 1889, of which the Open Court Publishing Company published an English translation by Prof. C. Stuart Gager in 1910. Freed from the hypothesis of the transportation of germs through the tissues, Darwin's pangenesis coincided with my own conception of the material basis of protoplasmic life and of the hereditary qualities. This study brought about the conviction that variability must at least consist in two essentially different principles. One of them is the origin of new qualities and their accumulation through geological times, producing the continuous development of higher forms from lower. This form is what we now call mutability. The other is our present fluctuating variability. It determines the degree in which the single qualities will show in different individuals. I proposed this difference between mutability and fluctuating variability at the conclusion of my book, but said to myself: It is all right to deduce the theoretical necessity of this conclusion, but it would be of far higher importance to prove the actual existence of these two types of variation.

I set at work at once, first in the field but soon in the garden. I cultivated over a hundred wild species, and some of them through many years. Fluctuating variability was everywhere present. Then I chanced to meet with Quetelet's *Anthropométrie,* which had appeared in 1870, applied his methods to plants and saw that here the same general laws prevail. Different forms of curves of variation were determined in the corn marigold (*Chrysanthemum segetum*) and other plants (1894-1899), and it became clear that they changed the properties only in the directions of more or less development, but gave no indication whatever of an origin of new qualities. Fluctuation and mutability must therefore be principally distinct.

Mutations must of course be rare, but some few of them occurred in my garden in well-guarded breeds. They were sudden, without visible preparation or transitions. The peloric toadflax appeared in 1894, the double corn marigold in 1896; they sufficed to prove the reality of mutations and gave an experimental basis for the appreciation and the study of the sudden appearance of new varieties in horticulture.

Besides them, one species proved to be rich in such sudden changes. It was Lamarck's evening primrose, a species originally wild in the eastern United States and collected there by Michaux, but which has since disappeared in America. It has, however, won an extensive distribution in England, Holland, Belgium and France, preferring the sand dunes along the coast. I observed its mutations for the first time in 1888 and since then it has never ceased to produce them. The number of mutants amounts to more than a dozen, some of them being progressive, as for instance the giant type or *Oenothera Lamarckiana gigas,* published in 1900, others retrogressive like the dwarfs and a brittle race called *O. rubrinervis.* Ordinarily they are constant from seed, but some show a splitting and are therefore considered to be half-mutants only, as *O. lata* and allied forms. The changes are always sudden and without transitions and occur so regularly in about 1% of the individuals that they constitute an unexpected but excellent material for experimental researches.

In my course on variability I laid especial stress on the pedigrees of definite systematic groups. The families of the euphorbiaceous and the umbelliferous plants afforded a very demonstrative material, and the hypothesis of the descent of the Monocotyls from the Dicotyls through types allied with the common buttercups, proposed at that time by Delpino, proved to be very convincing and instructive. Systematic atavisms, as shown in the leaf-bearing seedlings of the leafless species of Acacia and analogous instances were added to these discussions. They showed that evolution in nature is partly progressive and partly retrogressive. Progression means differentiation and specialization, it governs the main lines of the pedigree of th animal and vegetable kingdoms. But retrogression, consisting in the loss of previously developed qualities, mus be responsible for a large part of the diversity of forms i nature. And since it is easier to lose a thing than to acquir a new quality, the cases of retrogression must be far mor numerous in nature than those of actual progression.

Therefore there must be two kinds of mutations and even in our experimental cultures progressive ones must be rare, and retrogressive ones comparatively more frequent. This is exactly what we see in the mutations of the evening primrose.

Alongside of these studies I tried hybridization. Opium poppies afforded a useful material and led to the rediscovery of Mendel's law. At that time this conception was believed in by nobody, it was rather considered as an idealistic fiction. But the splitting of the poppies confirmed that of Mendel's peas, and numerous garden varieties behaved in the same way. I was fortunate enough to be the first to publish this result (1900) and pointed out that it is especially retrogressive variations which follow this law, whereas progressive ones produce constant hybrids, at least in many instances.

Paleontological studies strengthened the idea of the origin of species by means of sudden variations instead of a slow and gradual development. This side of the question has since been taken up by Charles A. White and other paleontologists. From my own studies I deduced the contention, that life on this earth has not lasted long enough for such a slow development as Darwin's theory of selection supposed. Darwin calculated some thousands of millions of years as required for his theory, but geologists and physicists only allow about forty or at most a hundred millions of years for the development of all animals and plants. The hypothesis of sudden mutations delivers us from this difficulty. And so it does for many other objections which were still being used as weapons against the whole principle of evolution in the form proposed by Darwin.

It has always been my conviction that the improvement of industrial practice is the main aim of all science. Biological science has to be a basis for agriculture and horticulture. The discipline of heredity should be crowned by the advance in our knowledge concerning the breeding of animals and plants. With Dr. Wakker I studied the diseases of the flower bulbs cultivated all around Haarlem (1883-1885), and since then I regularly sent contributions to the journal of our agricultural society. From 1892 to 1894 I was editor of the journal of the Dutch Horticultural Society in order to have an easy access to horticultural establishments in the Netherlands as well as abroad, and collected all the evidence I could find concerning practical plant-breeding. As a matter of fact this was very scanty but it led me to a connection with the Director of the Swedish agricultural station at Svalöf, Dr. Hjalmar Nilsson, whose celebrated method of plant improvement rested on the same scientific basis as my own experiments.

My book on the mutation theory is the combination of all these preliminary studies into a regular discussion of the main principle. I had the great advantage of my steadily repeated courses on heredity, which constituted, if I may say so, a first unpublished edition, with all the many faults inherent to first trials on a new field. The book appeared in 1900, and an English edition,[1] prepared by Prof. J. B. Farmer and A. D. Darbishire, was published by the Open Court Publishing Company in 1909. It tries to show that the origin of species is a natural phenomenon and that it is possible to subject it to experimental study. In nature the mutations have produced the whole evolution of all living beings; in the garden we can, of course, only expect to see their very smallest steps. The identity of retrogressive mutations in nature, in horticulture and agriculture and in the experimental garden seems now to be beyond doubt. But progressive changes, which are the most important, are at the same time the rarest, in nature as well as in cultivation. In regard to these the theory relies on its broad arguments and the question whether the directly observed progressive mutations afford a material for the interpretation of the ways of nature is still under discussion.

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[1] *The Mutation Theory.* 2 vols.

The theory is based upon arguments taken from widely different branches of nearly all natural sciences. It conduces of necessity to experimental research, but this, of course, is still in its first infancy. It promises, however, to become some day of important service to science at large as well as to the practice of breeders.

Hugo de Vries.

Lunteren, Holland.

THE MANUSCRIPTS OF LEIBNIZ ON HIS DISCOVERY OF THE DIFFERENTIAL CALCULUS.

PART II (CONTINUED).

§§ XI—XV.

Between the date of the manuscript last considered and the one which follows there is a gap of seven months, for which Gerhardt does not appear to have found anything. This is very unfortunate; for in this interval Leibniz has attained to the important conclusion that *the true general method of tangents is by means of differences.* We saw that in November 1675 he had *started* to investigate more thoroughly the direct method of tangents; but the method is that of the auxiliary curve, and there is no indication whatever of the characteristic triangle. Does this interval correspond with the time taken by Leibniz for his final reading of Barrow from Lect. VI to Lect. X, comparing all the geometrical theorems with his own notation? Or is it only a strange coincidence that Leibniz's order is the same as that of Barrow, first the auxiliary curve, and lastly the method of differences? One could form a more definite opinion, if Leibniz had given a diagram for the first problem he considers, the one in the next following manuscript, which amounts to the differentiation of an inverse sine. Such a diagram he must have had beside him as he wrote; for I think the reader will find that he wants one to follow the argument; with the idea of verifying this argument, I have not endeavored to supply the omission.

The consideration of the direct method of tangents is apparently, however, only as a means and not as an end; for Leibniz harks back to the inverse method, and to the catalogue of quadrible curves, which he seems to say he has in hand. It is not until November 1676 that he seems to be coming into his own; and it is not until July 1677 that he has a really definite statement of his rules. On the other hand, in July 1676, he is consistently using the differential factor with all his integrals, and before the end of that year he has the differential of a product, whether obtained as the inverse of his theorem $\int y\,dx = xy - \int x\,dy$, or by the use of the substitution $x + dx$, $y + dy$, is not certain; but this substitution appears in the manuscript for November 1676. Finally, in July 1677, appears the general idea of the substitution of other letters, in order to eliminate the difficulty caused by the appearance of the variable under a root sign or in the denominator of a fraction; and with this the whole thing is now fairly complete for all *algebraical functions*. There is as yet no equally clear method for the treatment of exponentials, logarithms, or trigonometrical functions; for the latter he refers to a geometrical diagram, strongly reminiscent of Barrow.

§ XI.

26 June, 1676.

Nova methodus Tangentium.
(New Method of Tangents.)

I have many beautiful theorems *with regard to the method of tangents both direct as well as inverse.* Descartes's method of tangents depends on finding two equal roots, and it cannot be employed, except in the case when all the undetermined quantities occurring in the work are expressible in terms of one, for instance, in terms of the abscissa.

But the true general method of tangents is by means of differences. That is to say, the difference of the ordinates, whether direct or converging, is required. It follows that quantities that are not amenable to any other kind of calculus are amenable to the calculus of tangents, so long as their differences are known. Thus if we are given an equation in three unknowns, in which x is an abscissa, y an ordinate, and z the arc of a circle of which x is the sine of the complement, e. g., the equation $b^2y=cx^2+fz^2$. To find the next consecutive y, in place of x take $x+\beta$, and in place of z take $z-dz$, or, since $\overline{dz}=\dfrac{\beta r}{\sqrt{r^2-x^2}}$, we may take $z-\dfrac{\beta r}{\sqrt{r^2-x^2}}$ (51); hence we have

$$b^2\,(y)=cx^2+2cx\beta+c\beta^2+fz^2-\frac{2fz\beta r}{\sqrt{r^2-x^2}}+\frac{\beta^2r^2}{r^2-x^2}$$

Hence the difference between y and (y) is given by

$$\pm b^2y\mp b^2(y)=+2cx\beta-\frac{2fz\beta r}{\sqrt{r^2-x^2}}=b^2\,\overline{dy}\,;$$

Therefore $$\frac{dy}{\beta}=\frac{\mp 2cx\sqrt{r^2-x^2}\mp 2fzr}{b^2\sqrt{r^2-x^2}}=\frac{t}{y}=\frac{tb^2}{cx^2+fz^2}.$$

From this the flexure or sinuosity of the curve can be found, according as now $2cz\sqrt{r^2-x^2}$, now $2fzr$ predominates; for when they are equal, the ordinate on that side on which it was previously the greater then becomes the less. It is just the same, if several other undetermined quantities, such as logarithms and other things occur, no matter how they are affected, as for instance in the equation $b^2y=cx^2+fz^2+xzl$, where z is supposed to be an arc, and l a logarithm, x the sine of the complement of the arc, and y the number of the logarithm, b being the radius and unity, equal to r. Also it is just the same, whenever an undetermined transcendental has been derived from some dimension or quadrature that has not been investigated.[52]

For the rest, many noteworthy and useful theorems now arise from the foregoing by the inverse method of tangents. Thus general equations, or equations of any indefinite degree may be formed, at first indeed in two unknowns, x and y, only. But if in this way the matter does not work out satisfactorily, it will easily do so when

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[51] In this and the following line I have corrected two obvious misprints; they are evidently not the fault of Leibniz, for the lines that follow from them are correct.

[52] There is some doubt here as to whether Leibniz could have given an example; but it must be remembered that these are practically only notes, mostly for future consideration.

the tables which I am investigating are finished; then it will be possible to take one or more other letters, and to take the difference as an arbitrary known formula, and when this is done it is certain that finally in any case a formula will be found such as is required, and in this way also a curve which will satisfy the conditions given; but in truth the description of the curve will need diagrams for these symbols, representing the sums of the arbitrarily chosen differences.

Now once a curve is found having the tangent property that we want, it will be more easy afterwards to find simpler constructions for it. We have this also as a convenient means enabling us to use many quantities that are transcendent, yet depending the one on the other, such for example as are all those that depend on the quadrature of the circle or the hyperbola. From these investigations it will also appear whether or no other quadratures can be reduced to the quadrature of the circle or the hyperbola. Lastly, since the finding of maxima and minima is useful for the inscription and circumscription of polygons, hence also, by employing these transcendent magnitudes, convergent series can be found, and in the same way their terminations; or of any quantities formed in the same way. However in that case it may not be so easy to argue about impossibility; at least indeed by the same method. Only I do not see how we can find whether from the quadrature of the circle, say, any sum can be found, when no quantity depending on the dimensions of the circle enters into the calculation.

§ XII.

July, 1676.

Methodus tangentium inversa.
[Inverse method of tangents.]

In the third volume of the correspondence of Descartes, I see that he believed that Fermat's method of Maxima and Minima is not universal; for he thinks (page 362, letter 63) that it will not serve to find the tangent to a curve, of which the property is that the lines drawn from any point on it to four given points are together equal to a given straight line.

[Thus far in Latin; Leibniz then proceeds in French.]

Mons. des Cartes (letter 73, part 3, p. 409) to Mons. de Beaune.

"I do not believe that it is in general possible to find the converse to my rule of tangents, nor of that which Mons. Fermat uses, although in many cases the application of his is more easy than mine; but one may deduce from it *a posteriori* theorems that apply to all curved lines that are expressed by an equation, in which one of the quantities, x or y, has no more than two dimensions, even if the other had a thousand. There is indeed another method that is more general and *a priori*, namely, by the intersection of two tangents, which should always intersect between the two points at which they touch the curve, as near one another as you can imagine; for in considering what the curve ought to be, in order that this intersection may occur between the two points, and not on this side or on that, the construction for it may be found. But there are so many different ways, and I have practised them so little, that I should not know how to give a fair account of them."

Mons. des Cartes speaks with a little too much presumption about posterity; he says (page 449, letter 77) that his rule for resolving in general all problems on solids has been without comparison the most difficult to find of all things which have been discovered in geometry up to the present, and one which will possibly remain so after centuries, "unless I take upon myself the trouble of finding others" (as if several centuries would not be capable of producing a man able to do something that would be of greater moment).

(Page 459.) The question of the four spheres is one that is easy to investigate for a man who knows the calculus. It is due to Descartes, but as it is given in the book, it appears to be very prolix.

The problem on the inverse method of tangents, which Mons. des Cartes says he has solved (Vol. 3, letter 79, p. 460)

[Leibniz then continues in Latin.]

EAD is an angle of 45 degrees. ABO is a curve, BL a tangent to it; and BC, the ordinate, is to CL as N is to BJ. Then

$$\mathrm{CL}=\frac{\mathrm{BC}=ny}{\mathrm{BJ}=y-x},\ \mathrm{CL}=t,$$

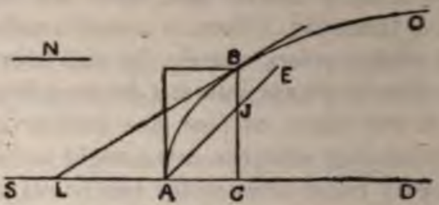
hence, $$t=\frac{ny}{y-x},\ \frac{n}{t}=\frac{y-x}{y}=1-\frac{x}{y},$$

hence, $$\frac{x}{y}=\frac{t-n}{t};\ \text{but}\ \frac{t}{y}=\frac{\overline{dx}}{dy};$$

therefore $$\frac{\overline{dx}}{dy}=\frac{n}{y-x},\ \text{or}\ \overline{dx}\,y-x\,\overline{dx}=\overline{dy}\,n;$$

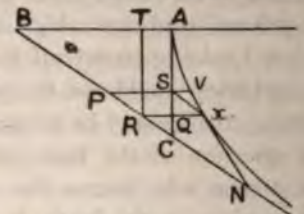
hence $$\int\overline{dx}\,y-\int x\,\overline{dx}=-n\int dy.$$

Now, $\int \overline{dy}=y$, and $\int \overline{x\,dx}=x^2/2$, and $\int \overline{dx\,y}$ is equal to the area ACBA, and the curve is sought in which the area ACBA is equal to $(x^2/2)+ny=(\mathrm{AC}^2/2)+n\mathrm{BC}$. [53]



Let this $x^2/2$, i. e., the triangle ACJ be cut off from the area, then the remainder AJBA should be equal to the rectangle ny.

The line that de Beaune proposed to Descartes for investigation reduces to this, that if BC is an asymptote to the curve, BA the axis, A the vertex, AB, BC, fixed lines, for BAC is at right angles.



Let RX be an ordinate, XN a tangent, then RN is always to be constant and equal to BC; required the nature of the curve.

This is how I think it should be done.

Let PV be another ordinate, differing from the other one RX by a straight line VS, found by drawing XS parallel to RN; then the triangles SVX, RXN are similar, RN $=t=c$, a constant, RX $=y$, SY $=dy$, and therefore

---

[53] Leibniz has a footnote to this manuscript: "I solved in one day two problems on the inverse methods of tangents, one of which Descartes alone solved, and the other even he owned that he was unable to do."

This problem is one of them, the first mentioned in the footnote given by Leibniz. But it requires a stretch of imagination to consider Leibniz's result as a solution. For he ends up with a geometrical construction, that is at least as hard as the construction that can be made by the use of the original data. There is of course the usual misprint that one is becoming accustomed to; but there is also the unusual, for Leibniz, mistake of using his data incorrectly. Starting with the hypothesis that $\mathrm{BC}:\mathrm{CL}=\mathrm{N}:\mathrm{BJ}$, he writes $\mathrm{CL}=\mathrm{N}.\mathrm{BC}/\mathrm{BJ}$ (correcting the omission of the factor N), instead of $\mathrm{CL}=\mathrm{BC}.\mathrm{BJ}/\mathrm{N}$.

The solution of the problem is $y+n\log(y-x+n)=0$, as originally stated, or $x=n\log(n-y+x)$, if we continue from Leibniz's erroneous result $dx/dy=n/(y-x)$.

The point to be noted, however, is that Leibniz does not remark that "this curve appertains to a logarithm."

$$\frac{\overline{dy}}{dx}=\frac{y}{t=c}\ ;\ \text{hence } cy=\int\overline{y\,\overline{dx}}\quad\text{or } c\,\overline{dy}=y\,\overline{dx}.$$ [54]

If AQ or TR $=z$, and AC $=f$, while BC $=a$;

then, $$\frac{\text{AC}}{\text{BC}}=\frac{f}{a}=\frac{\text{TR}}{\text{BR}}=\frac{z}{x};\ \text{and thus } x=\frac{az}{f}.$$

If $\overline{dx}$ is constant, then $\overline{dz}$ is also constant. Hence

$c\,dy=\frac{a}{f}y\,\overline{dz}$, or $cy=\frac{a}{f}\int\overline{y\,\overline{dz}}$, and $cy\,\overline{dy}=\frac{a}{f}y^2\,\overline{dz}$, therefore

$c\frac{y^2}{2}=\frac{a}{f}\int y^2\,\overline{dz}$. Hence we have both the area of the figure and the moment to a certain extent (for something must be added on account of the obliquity); also

$cz\,\overline{dy}=\frac{a}{f}yz\,\overline{dz}$, and therefore $c\int z\,\overline{dy}=\frac{a}{f}\int\overline{yz\,\overline{dz}}$.

Also $\frac{c\overline{dy}}{y}=\frac{a}{f}dz$, and hence, $c\int\frac{\overline{dy}}{y}=\frac{a}{f}z$. Now, unless I am greatly mistaken, $\int\frac{\overline{dy}}{y}$ is in our power.[55] The whole matter reduces to this, we must find the curve[56] in which the ordinate is such that it is equal to the differences of the ordinates divided by the abscissae, and then find the quadrature of that figure.

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[54] Leibniz does not see that this result immediately gives him the equation that he requires. Thus $x=c\,\mathrm{Log}\,y$, as he would have written it; the usual omission of the arbitrary constant does not matter in this case, so long as BA is taken as unity, which is possible with Leibniz's data.

[55] Here he seems to recognize that he has the solution. The next sentence is, however, very strange. As long ago as Nov. 1675 he has written $\int a^2/y$ as Log y, and recognized the connection between the integral and the quadrature of the hyperbola; and yet he says "unless I am mistaken, $\int dy/y$ is always in our power." Now notice that in the date there is no day of the month given, contrary to the usual custom with these manuscripts so far; can it be possible that this date was afterward added from memory, and that the manuscript should bear an earlier date? If not we must conclude that Leibniz has not yet attained to a correct idea of the meaning of his integral sign, and is still worried by the necessity (as it appears to him) of taking the y's in arithmetical progression.

[56] The passage in the original Latin is very ambiguous, and it may be that it is not quite correctly given; I think, however, that I have given the correct idea of what Leibniz intended. One has to draw an *auxiliary* curve, in which $y=dy/dx$, and then find its area; in that case it should be "divided by the differences of the abscissae" instead of "divided by the abscissae."

$$\overline{d\sqrt{ay}} = \frac{1}{\sqrt{ay}}$$ (57)

Figures of this kind, in which the ordinates are dy/y, dy/y^2 dy/y^3, are to be sought in the same way as I have obtained those whose ordinates are $y\,dy$, $y^2\overline{dy}$, etc. Now $w/a = \overline{dy}/y$, and since $\overline{dy}$ may be taken to be constant and equal to β,[58] therefore the curve, in which $w/a = \overline{dy}/y$, will give $wy = a\beta$, which would be a hyperbola.[59] Hence the figure, in which $dy/y = z$, is a hyperbola, no matter how you express y, and if y is expressed by ϕ^2 we have $dy = 2\phi$, and $\frac{2\phi}{\phi^2} = \frac{2}{\phi}$. Now, $c\int\frac{dy}{y} = \frac{a}{f}z$, and therefore $\frac{fc}{a}\int\frac{1}{y} = z$, which thus appertains to a logarithm.[60]

Thus we have solved all the problems on the inverse method of tangents,[61] which occur in Vol. 3 of the Correspondence of Descartes, of which he solved one himself, as he says on page 460, letter 79, Vol. 3; but the solution is not given; the other he tried to solve but could not, stating that it was an irregular line, which in any case was not in human power, nay not within the power of the angels unless the art of describing it is determined by some other means.

§ XIII.

This manuscript bears no date: however, it was probably written very shortly after his call on Hudde at Amsterdam, on his way home from England (the second visit)

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[57] An interpolated note, marking a sudden thought or guess; for the next sentence carries on the train of thought that has gone before. Query, some interval of time, either short (such as for a meal) or long (continued the next day), may have occurred here.

[58] This cannot be referred back to the present problem, since Leibniz has already assumed in it that dz and dx are constant. This may account for the fact that he has hesitated to say that the integral represents a logarithm.

[59] This working is intended to apply to the auxiliary curve mentioned above, w standing for dx, and β for dy; hence the curve is not a hyperbola; Leibniz seems to have been misled by the appearance of the equation suggesting $xy =$ constant.

[60] Here apparently he leaves the muddle, in which he has entangled himself, and returns to his original equation; he then remembers that he has found before that the integral in question leads to a logarithm.

[61] He has not solved either of them; nor can it be said from this that "Leibniz in 1676 sought and found the curve whose subtangent is constant." Of all the work that Leibniz has done hitherto, there is none that is so inconclusive as this in comparison.

to Hanover. Leibniz stayed in Holland from October 1676 to December of that year; hence the date may be fairly accurately assigned.

Hudde showed me that in the year 1662 he already had the quadrature of the hyperbola, which I found was the very same as Mercator also had discovered independently, and published. He showed me a letter written to a certain van Duck, of Leyden I think, on this subject. His method of tangents is more complete than that of Sluse, in that he is able to use any arithmetical progression, as in a simple equation, whereas Sluse and others can use only one. Hence constructions can be made simple, while terms can be eliminated at will. This also can be made use of for eliminating any letter with greater facility, for numerous equations of all sort are thereby rendered fit for elimination.

$$\begin{array}{llll}
x^3 + & px^2 + & qx & = 0 \\
 & y & y.\ y & \\
 & & y^2.\ y^2 & \\
 & & y^3 & \\
\hline
3x^2 + & 2px^2 + & qx & 0 \\
 & 2yx^2 + & yx & \\
 & & y^2x &
\end{array}
\qquad
\begin{array}{l}
x^2 + xy + y^2 + x + y + \mathrm{a} = 0 \\
2x\overline{dx} + x\overline{dy} + 2y\overline{dy} + \overline{dx} + \overline{dy} \quad = 0 \\
\qquad y\overline{dx} \\
\dfrac{t}{y} = \dfrac{\overline{dx}}{\overline{dy}} = \dfrac{x + 2y + 1}{y + 2x + 1}
\end{array}$$

What I had observed with regard to triangular numbers for three equal roots, and pyramidal numbers for four, was already known to him, and indeed even more generally,

$$\begin{array}{rrrrrrrrrr}
 & & -1 & 0 & 1 & 2 & 3 & 4 & 5 & 6 \\
 & -3 & -1 & 0 & 0 & 1 & 3 & 6 & 10 & 15 \\
-4 & -1 & 0 & 0 & 0 & 1 & 4 & 10 & 20 &
\end{array}$$

Here it must be observed that the number of zeros increases, as this is of the greatest service in separating roots.

He has also rules for multiplying equations, so that they are not only determined for equal roots, but also for roots increasing arithmetically, or geometrically, or according to any progression.

Hudde has a most elegant construction for describing two curves, one outside and the other inside a circle, which are capable of quadrature, and by means of these curves he finds the true area of a circle so nearly, that with the help of the dodecagon, in a number of six figures, there is an error of only three units, or 3/100000.

He has a method for finding the real roots of equations, having some roots real and the rest impossible, by the help of another equation having all its roots real, and as many in number as he previously had of real and impossible together.

He had an example of a beautiful method of finding sums of series by the continuous subtractions of geometrical progressions. He subtracts geometrical progressions whose sums are also geometrical progressions, and thus he can find the sums of the sums, and so he obtains the sum of the series. This method is excellent for a series whose numerators are arithmetical, and denominators geometrical, such as,

$$\frac{1}{2}\ \frac{2}{4}\ \frac{3}{8}\ \frac{4}{16}\ \cdots\cdots\ .$$

He has three series, like those of Wallis, for interpolations for the circle. He says that there are no more by that method, I think.

Also he can very often write down the quadratures of irrationals, as also their tangents, without eliminating irrationals, or fractions, etc.

§ XIV.

November, 1676.

Calculus Tangentium differentialis.
[Differential calculus of tangents.]

$$\overline{dx}=1,\quad \overline{dx^2}=2x,\quad \overline{dx^3}=3x^2,\quad \text{etc.}$$

$$\overline{d\frac{1}{x}}=-\frac{1}{x^2},\quad \overline{d\frac{1}{x^2}}=-\frac{2}{x^2},\quad d\frac{1}{x^3}=\frac{3}{x^2},\quad \text{etc.}$$

$$\overline{d\sqrt{x}}=\frac{1}{\sqrt{x}},\quad \text{etc.}$$

From these the following general rules may be derived for the differences and sums of the simple powers:

$$\overline{dx^e}=e,x^{e-1}\text{, and conversely }\int\overline{x^e}=\frac{x^{e+1}}{e+1}.$$

Hence, $\overline{d\frac{1}{x^2}}=\overline{dx^{-2}}$ will be $-2x^{-3}$ or $-\frac{2}{x^3}$,

and $\overline{d\sqrt{x}}$ or $dx^{\frac{1}{2}}$ will be $-\frac{1}{2}x^{-\frac{1}{2}}$ or $-\frac{1}{2}\sqrt{\frac{1}{x}}$.

Let $y=x^2$, then $\overline{dy}=2x\,\overline{dx}$ or $\frac{\overline{dy}}{dx}=2\,x$.

This reasoning is general, and it does not depend on what the progression for the x's may be.[62] By the same method, the general rule is established as:

$$\frac{\overline{dx^e}}{dx}=e\,x^{e-1},\ \text{and]}\ \int\overline{x^e\,\overline{dx}}=\frac{x^{e+1}}{e+1}.$$

Suppose that we have any equation whatever, say,

$$ay^2+byx+cz^2+f^2x+g^2y+h^3=0,$$

and suppose that we write $y+dy$ for y, and $x+dx$ for x, we have, by omitting those things which should be omitted, another equation

$$\left.\begin{array}{l} ay^2+byx+cx^2+f^2x+g^2y+h^3=0 \\ \hline a2\overline{dy}y+by\overline{dx}+2cx\overline{dx}+f^2\overline{dx}+g^2\overline{dy} \\ \qquad\quad bx\overline{dy} \\ \hline a\,\overline{dy}^2+b\,\overline{dx}\,\overline{dy}+c\,\overline{dx}^2=0 \end{array}\right\}=0 \quad ^{(63)}$$

This is the origin of the rule published by Sluse. It can be extended indefinitely: Let there be any number of letters, and any formula composed from them; for example, let there be the formula made up of three letters,

$$ay^2\quad bx^2\quad cz^2\quad fyx\quad gyx\quad hxz\quad ly\quad mx\quad nz\quad p=0.$$

From this we get another equation

$$\begin{array}{ccccccccc} ay^2 & bx^2 & cz^2 & fyx & \text{simi-} & ly & mx & \text{simi-} & p \\ \hline 2a\overline{dy}y & 2b\overline{dx}x & 2c\overline{dz}z & fy\overline{dx} & \text{larly} & l\overline{dy} & m\overline{dx} & \text{larly} & \\ & & & fx\overline{dy} & & & & & \\ \hline a\,\overline{dy}^2 & b\overline{dx}^2 & c\overline{dz}^2 & f\overline{dx}\overline{dy} & \ldots\ldots & & & & \end{array}$$

It is plain from this that by the same method tangent planes to surfaces may be obtained, and in every case that it does not matter whether or no the letters x, y, z have any known relation, for this can be substituted afterward.

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[62] AT LAST! The recognition of the fact that neither dx nor dy need necessarily be constant, and the use of another letter to stand for the function that is being differentiated, mark the beginning, the true beginning, of Leibniz's development of differentiation. Later in this manuscript we find him using the third great idea, probably suggested by the second of those given above, namely, the idea of substitution, by means of which he finally attains to the differentiation of a quotient, and a root of a function.

It is very suggestive that this remarkable advance occurs after his second visit to London, while he is staying in Holland. Did some one tell then of the work of Newton, or of Barrow's method (which is geometrically an exact equivalent of substitution), pointing out those things of which he had not perceived the drift, or is it the result of his intercourse with Hudde? For the date is that of his stay at The Hague. (For the answer to this query see an article to follow, entitled "Leibniz in London."—ED.)

[63] This is Barrow all over; even to the words *omissis omittendis* instead of Barrow's *rejectis rejiciendis.* Lect. X, Ex. 1 on the differential triangle at the end of the lecture.

Further, the same method will serve admirably, even though compound fractions or irrationals enter into the calculation, nor is there any need that other equations of a higher degree should be obtained for the purpose of getting rid of them; for their differences are far better found separately and then substituted; hence the ordinary method of tangents will not only proceed when the ordinates are parallel, but it can also be applied to tangents and anything else, aye, even to those things that are related to them, such as proportions of ordinates to curves, or where the angle of the ordinates changes according to some determined law. It will be worth while especially to apply the method to irrationals and compound fractions.[64]

$$\overline{d\sqrt[2]{a+bz+cz^2}}.\quad \text{Let } a+bz+cz^2=x;$$

then $$\overline{d\sqrt[2]{x}}=-\frac{1}{2\sqrt{x}},\ \text{and}\ \frac{dx}{dz}=b+2cz;$$

therefore $$\overline{d\sqrt[2]{a+bz+cz^2}}=-\frac{b+2cz}{2\,dz\sqrt{a+bz+cz^2}}$$

Taking any equation between two letters x and y for a curve, and determining the equation of the tangent, either of the two letters x or y can be eliminated, so that all that remains is the other together with $\overline{dx}$ and $\overline{dy}$; and this will be worth while doing in all cases to facilitate the calculation.

If three letters are given, say x, y and z, and the value of $\overline{dz}$ is expressed in terms of x or y (or even of both), an equation for the tangents will at length be obtained, in which again there will be left only one or other of the letters x or y together with the two, $\overline{dx}$ and $\overline{dy}$; sometimes z itself cannot be eliminated. Also this can be deduced in all cases of an assumed value of $\overline{dz}$, and in the same way more additional letters can be taken. Thus, bringing together every general calculus into one, we obtain the most general of them all. Besides, the assumption of a large number of letters may be employed to solve problems on the inverse method of tangents, with the assistance of quadratures.

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[64] Here we have the idea of substitutions, which made the Leibnizian calculus so superior to anything that had gone before. Note that he still has the erroneous sign that he obtained for the differentiation of $\sqrt{x}$ at the beginning of this manuscript. Also that the dz is wrongly placed in the denominator of the result.

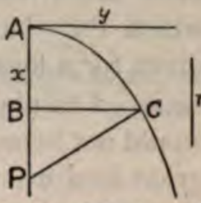
Thus, if the following problem is set for solution: It is given that the sum of the straight lines CB, BP or

$$y + y\frac{\overline{dy}}{dx} = xy\,;$$

we have

$$\overline{dx} + \overline{dy} = x\overline{dx}^{\,*}$$

$$\text{or } x + y = \frac{x^2}{2}.$$



Thus we have the curve in which the sum of CB + BP (multiplied by a constant r) is equal to the rectangle AB.BC.

There are two marginal notes by Leibniz that must be referred to, in this manuscript. The first reads:

It is especially to be observed about my calculus of differences that, if

$$b, ydx + xdy + \text{etc.} = 0$$

then $byx + \int$ etc. $= 0$, and so on for the rest. It is to be seen what is to be done about the h^3. For the purpose of making these calculations better, the equation $ay^2 + byx + cx^2 +$ etc. can be changed into something else by means of another relation of the curve, and if it turns out all right it may be compared to another calculation of the differences, since it comes to the thing as by the first. The two points to be noticed are that Leibniz now for the first time recognizes the need of considering the arbitrary constant of integration, though he hardly grasps how it arises, and that even now he cannot refrain from harking back to his obsession of the obtaining of several equations for comparison. This note is not made any the easier to understand by its being starred by Gerhardt for reference to the differentiation of x^2, whereas it obviously (when you come later to the passage) refers to the differentiation of the equation of the second degree.

The second note refers to the substitution of $x + dx$ for x and $y + dy$ for y, and reads:

Either dx or dy can be expressed arbitrarily, a new equation being obtained; and either dx or dy being taken away, x, or y, say, can be otherwise expressed in terms of the quantities. It is not true, I think, that this is so, for then a catalogue of all curves capable of quadrature would result, by supposing one or other of them to be constant.

The point to be noticed in this rather ambiguous statement is that Leibniz is still thinking of his catalogue, and is not himself convinced of the completeness of his method for all purposes.

§ XV.

There is an interval of nearly seven months between the date of the manuscript last considered and the one that now follows. This interval has been full of work; for we now find a clear exposition of the rules for the differentiation of a sum, difference, product, quotient, etc., though these are without proof, or indication of the manner in which they have been obtained. There is also no rule given for a logarithm, an exponential, or a trigonometrical ratio. Leibniz may have known them, but even then it would not be surprising to find them left out; for Leibniz's great idea was the use of his method to facilitate calculation. We must conclude therefore that these rules are a development of the method of substitution outlined in the preceding manuscript.

This essay has several peculiar characteristics of its own, which distinguish it from those that have gone before. It is written throughout in French; it is to some extent historical and critical, having the appearance of being prepared for publication, or possibly as a letter; this is corroborated by the fact that there is an original draft and a more fully detailed revision. Could it be that this is the original of Leibniz's communication of this method to Newton and others? If so, Leibniz is very careful not to give much away. The figures are strongly reminiscent of Barrow, but the context does not deal with subtangents, which are such a feature in all Barrow's work.

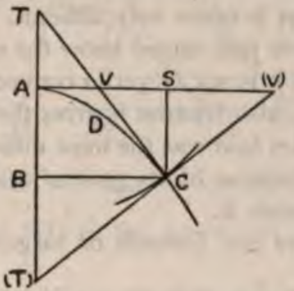
The start from the work of Sluse is peculiar; it seems to suggest that Leibniz is pointing out that his method is a fuller development of that of the former. Leibniz has already hazarded two different guesses at the origin of the rules given by Sluse; the second, namely, by substitution of $x + dx$ for x, etc., being the more probable. Is Leibniz trying to draw a red herring across the trail, the real trail that leads to Barrow's a and e?

11 July 1677.

Méthode générale pour mener les touchantes des Lignes Courbes sans calcul, et sans réduction des quantités irrationelles et rompues.

[General method for drawing tangents to curves without calculation, and without reducing irrational or fractional quantities.]

Slusius has published his method of finding tangents to curves without calculation, in which the equation is purged of irrational or fractional quantities.



For example, a curve DC being given, in which the equation expresses the relation between BC and AS, which we will call y, and AB or SC, which we will call x; let this be

$$a+bx+cy+dxy+ex^2+fy^2+gx^2y+hxy^2+kx^3+ly^3+\text{etc.}=0.$$

One has only to write

$$\begin{array}{llllllllll} 0 & +\,b\xi & +\,cv & +\,dxv & +\,2ex\xi & +\,2fyv & +\,gx^2v & +\,hy^2\xi & +\,3kx^2\xi & +\,3ly^2v \\ & & & dy\xi & & & 2gxy\xi & 2hxyv & & \end{array}$$

$$\begin{array}{lllll} +\,mx^2y^2 & +\;nx^3y & +\;pxy^3 & +\;qx^4 & +\;ry^4 \qquad (65) \\ +\,2mx^2yv & +\;nx^3v & +\;py^3\xi & +\,4qx^3\xi & +\,4ry^3v \\ +\,2mxy^2\xi & +\,3nx^2y\xi & +\,3\,py^2xv & & \end{array}$$

that is to say, if the equation is changed to a proportion,

$$\frac{\xi}{v}=\frac{c+dx+2fy+gx^2+2hxy+3ly^2+2mx^2y+\text{etc.}}{b+dy+2ex+2gxy+hy^2+3kx^2+\text{etc.}};$$

and, supposing that $\frac{\xi}{v}$ expresses the ratio $\frac{\text{TB}}{\text{BC}=x}$ or $\frac{\text{CS}=y}{\text{SV}}$, then TB or SV can be obtained, if BC and SC are supposed to be given. When the given magnitudes, b, c, d, e, etc., with their proper signs, make the value of ξ/v a negative magnitude, the tangent will not be CT which goes toward A, the start of the abscissa AB, but C(T) which goes away from it. That is all that has been

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[65] This line represents the "etc." of the original equation, and is set down for the purpose of getting the derived terms; the complete derived equation therefore consists of the two lines above and the two below. Note the omission of the negative sign, when changing from the equation to the proportion.

published up to the present time, easy to understand by any one that is versed in these matters. But when there are irrational or fractional magnitudes, which contain either x or y or both, this method cannot be used, except after a reduction of the given equation to another that is freed from these magnitudes. But at times this increases to a terrible degree the calculation and obliges us to rise to very high dimensions, and leads us to equations for which the process of depression is often very difficult. I have no doubt that the gentlemen[66] I have just named know the remedy that it is necessary to apply, but as it is not as yet in common use, and is I believe known to but a few, also because it gives the finishing touch to the problem that Descartes said was the most difficult to solve of all geometrical problems, because of its general utility, I have thought it a good thing to publish it.

Suppose we have any formula or magnitude or equation such as was given above,

$$a+bx+cy+dxy+ex^2+fy^2+\text{etc.};$$

for brevity let us call it ω; that which arises from it when it is treated in the manner given above, namely,

$$b\xi+cv+dxv+dy\xi+\ \text{etc.};$$

will be called $d\omega$; and in the same way, if the formula is λ or μ, then the result above will be $d\lambda$ or $d\mu$, and similarly for everything else. Now let the formula or equation or magnitude ω be equal to λ/μ, then I say that $d\omega$ will be equal to $\frac{\mu\overline{d\lambda}-\lambda\overline{d\mu}}{\mu^2}$. This will be sufficient to deal with fractions.

Again, let ω be equal to $\sqrt[z]{\omega}$, then $d\omega = \frac{dw}{z.\sqrt[z]{\omega^{z-1}}}$; and this will be sufficient for the proper treatment of irrationals.

Algorithm of the new analysis for maxima and minima, and for tangents.

Let $AB=x$, and $BC=y$, and let TVC be the tangent to the curve AC; then the ratio $\frac{TB}{BC=y}$ or $\frac{SC=x}{SV}$ will be called $\frac{dx}{dy}$.

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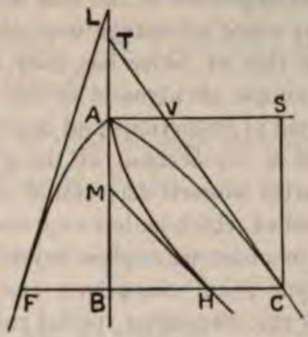
[66] Leibniz, at the beginning, first wrote, "Hudde, Sluse, and others"; but later he struck out all but Sluse. (Gerhardt.)

Let there be two or more other curves, AF, AH, and suppose



that BF $=v$ and BH $=w$, and that the straight line FL is the tangent to the curve AF, and MH to the curve AH; also $\frac{\mathrm{LF}}{\mathrm{FB}} = \frac{dx}{dv}$, and $\frac{\mathrm{MH}}{\mathrm{BH}} = \frac{dx}{dw}$; then I say that dy, or dvw, will be equal to $vdw+wdv$; and if $v=w=x$, and $y=vw=x^2$, then by substituting x for v and for w, we shall have $dvw=2xdx$.

(This will also hold good if the angle ABC is either acute or obtuse; also if it is infinitely obtuse, that is to say, if TAC is a straight line.)

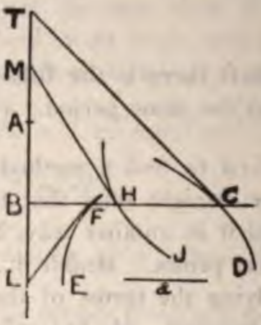
[Of this rough draft there is the following revision, and this obviously comes within the same period. (Gerhardt.)]

Fermat was the first to find a method which could be made general for finding the straight lines that touch analytical curves. Descartes accomplished it in another way, but the calculation that he prescribes is a little prolix. Hudde has found a remarkable abridgment by multiplying the terms of the progression by those of the arithmetical progression. He has only published it for equations in one unknown; although he has obtained it for those in two unknowns. Then the thanks of the public are due to Sluse; and after that, several have thought that this method was completely worked out. But all these methods that have been published suppose that the equation *has been reduced* and cleared of fractions and irrationals; I mean of those in which the variables occur. I however have found means of obviating these useless reductions, which make the calculation increase to a terrible degree, and oblige us to rise to very high dimensions, in which case we have to look for a corresponding depression with much trouble; instead of all this, everything is accomplished at the first attack.

This method has more advantage over all the others that have been published, than that of Sluse has over the rest, because it is one thing to give a simple abridgment of the calculation, and quite another thing to get rid of reductions and depressions. With respect to the publication of it, on account of the great extension of the matter which Descartes himself has stated to be the most useful part of Geometry, and of which he has expressed the hope that there is more to follow—in order to explain myself shortly and clearly, I must introduce some *fresh characters,* and give to them a *new Algorithm,* that is to say, altogether special rules, for their addition, subtraction, multiplication, division, powers, roots, and also for equations.

Explanation of the characters.

Suppose that there are several curves, as CD, FE, HJ, connected with one and the same axis AB by ordinates drawn through one and the same point B, to wit, BC, BF, BH. The tangents CT, FL, HM to these curves cut the axis in the points T, L, M; the point A in the axis is fixed, and the point B changes with the ordinates. Let $AB = x$, $BC = y$, $BF = w$, $BH = v$; also let the ratio of TB to BC be called that of dx to dy, and the ratio of LB to BF that of dx to dw, and the ratio of MB to BH that of dx to dv. Then if, for example, y is equal to vw, we should say dvw instead of dy, and so on for all other cases. Let a be a constant straight line; then, if y is equal to a, that is, if CD is a straight line parallel to AB, dy or da will be equal to 0, or equal to zero. If the magnitude dx/dw comes out negative, then FL, instead of being drawn



toward A, above B, will be drawn in the contrary direction, below B.

Addition and Subtraction. Let $y = v \pm w (\pm) a$, then $\overline{dy}$ will be equal to $\overline{dv} \pm \overline{dw} (\pm) 0$.

Multiplication. Let y be equal to avw, then $\overline{dy}$ or $\overline{davw}$ or $a\,\overline{dvw}$ will be equal to $av\,\overline{dw} + aw\,\overline{dv}$.

Division. Let y be equal to $\frac{v}{aw}$, then $\overline{dy}$ or $\overline{d\frac{v}{aw}}$

$$\text{or } \frac{1}{a}\overline{d\frac{v}{w}} \text{ will be equal to } \frac{w\,\overline{dv} - v\,\overline{dw}}{aw^2}.$$

The rules for *Powers* and *Roots* are really the same thing.

Powers. If $y = w^z$, (where z is supposed to be a certain number), then $\overline{dy}$ will be equal to z, w^{z-1}, dw.

Roots or extractions. If $y = \sqrt[z]{w}$, then $\overline{dz} = \frac{dw}{z\sqrt[z]{w^{z-1}}}$.

Equations expressed in rational integral terms.

$$a + bv + cy + tvy + ev^2 + fy^2 + gv^2y + hvy^2 + kv^3 + ly^3$$
$$+ mv^2y^2 + nv^3y + pvy^3 + qv^4 + ry^4 = 0,$$

supposing that a, b, c, t, e, etc. are magnitudes that are known and determined; then we should have

$$\begin{aligned} 0 = b\overline{dv} + c\overline{dy} + tv\overline{dy} + 2ev\overline{dv} + 2fy\overline{dy} + {} & gv^2\overline{dy} + hy^2\overline{dv} \\ ty\overline{dv} \qquad\qquad\qquad & +2gvy\overline{dy} + 2hvy\overline{dy} \end{aligned}$$

$$\begin{aligned} + 3ly^2\overline{dy} + 2mv^2y\overline{dy} + nv^3\overline{dy} + py^3\overline{dv} + 4qv^3\overline{dv} + 4ry^3\overline{dy} \\ + 2mvy^2\overline{dv} + 3nv^2y\overline{dv} + 3py^2v\overline{dy} \end{aligned}$$

This rule can be proved and continued without limit by the preceding rules; for, if

$$a + bv + cy + tvy + ev^2 + fy^2 + gv^2y + \text{etc.} = 0,$$

then $da + dbv + dcy + tdvy + edv^2 + fdy^2 + gdv^2y + \text{etc.}$ will also be equal to 0. Now $da = 0$, $dbv = bdv$, $dcy = cdy$, $dvy = vdy + ydv$; also $dv^2 = 2vdv$, since dv^z is equal to z, v^{z-1}, dv, that is to say (by substituting 2 for z) $2vdv$; and $dv^2y = v^2dy + 2vydv$, for, supposing that $w = v^2$, then dv^2y will be dwy, and $dwy = ydw + wdy$, and dw or $dv^2 = 2vdv$; hence in the value of dwy, substituting for w and dw the values found for them, we shall have $dv^2y = v^2dy + 2vydv$, as obtained above. This can go on without limit. If in the given equation $a + bv + cy$ + etc. $= 0$, the magnitude v were equal to x, that is to say if the line JH were a straight line which when produced passed through the point A, making an angle of 45 degrees with the axis, then the resulting equation, transformed into a proportion, would give the rule for the method of tangents, as published by Sluse; and, in consequence, this is nothing but a particular case or corollary of the general method.

Equations complicated in any manner with fractions and irrationals. These could be treated in the same way without any calculation, by supposing that the denominator of the fraction or the magnitude of which it is necessary to take the root is equal to a magnitude or letter, which is to be treated according to the preceding rules.[67]

Also, when there are magnitudes which have to be multiplied by one another, there is no need to make this multiplication in reality, which saves still more labor. One example will be sufficient.

[No example is given, however; but the following seems to have been added later, according to Gerhardt.]

Lastly this method holds good when the curves are not purely analytical, and even when their nature is not expressed by such ordinates, and in addition it gives a marvelous facility for making geometrical constructions. The true reason for an abridgment so admirable, and one that enables us to avoid reductions of fractions and irrationals, is that one can always make certain, by means of the preceding rules, that the letters dy, dv, dw, and the like, shall not occur in the denominator of the fraction, or under the root-sign.

§ XVI.

The next manuscript appears to be a more detailed revision of the one last considered. It bears no date; but it is safe to say that it belongs to a considerably later period than that of July 1677. For in this are given, by means of the *infinitely small* quantities dx and dy, proofs of the fundamental rules for the first time; the figure notation is changed from the clumsy C, (C), ((C)) to the neat ${}_1C$, ${}_2C$, ${}_3C$; the notation for proportion is now $a:b::c:d$; and there are several other changes that readers will notice as they go along. The ideas of Leibniz are now approaching crystallization, as is evidenced by the fact that $\int y\,dx$ is clearly stated for the first time to be the sum of *rectangles made from y and dx.* It is rather astonishing, however, in this connection to find $\int \overline{x+y-v} = \int x + \int y - \int v$, which can have no significance according to the above definition; and also to find the whole thing explained by arithmetical series, in which however it is to be observed that dx is not taken to be constant. But for this one might almost place this later than the publication of the method in the *Acta Eruditorum* in 1684; in this essay Leibniz gave a full account of his rules without proofs, and is evidently trying to get away from the idea of the infinitely small, an effort which culminates in the next, and last, manuscript of this set.

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[67] The complete statement of the method of substitutions.

If then we guess the date to be about 1680, probably we shall not be very far out.

A remarkable feature of this manuscript is the omission of really necessary figures, without which the text is very hard to follow. Of course this manuscript was written for publication, and the suggestion may be made that the diagrams were drawn separately, just as in books of that time they were printed separately on folding plates; but then, why has he given three diagrams? The only other suggestion that can be made as far as I can see is that he was referring to texts, in which the diagrams were already drawn, by Gregory St. Vincent, Cavalieri, James Gregory (one of whose theorems he quotes), Barrow (who strangely enough also quotes the very same theorem), Wallis, and others. For he mentions many of these authors, but there is never a word about Barrow. I consider that he was looking up their theorems to show *how much superior his method was to any of theirs.*

It is to be observed that not even in this manuscript is there any mention of logarithms, exponentials, or trigonometrical ratios. We shall see later that Leibniz is reduced to obtaining the integral of $(a^2 + x^2)^{1/2}$ by reference to a figure and its quadrature; that is to say, he is apparently unable to perform the integration analytically. It therefore follows that, if he got a great deal from Barrow, he was unable to understand the Lect. XII, App. I of the *Lectiones Geometricae.*

The final conclusion that I personally have come to, after completing this examination of the manuscripts of Leibniz, as far as they are given by Gerhardt is this:

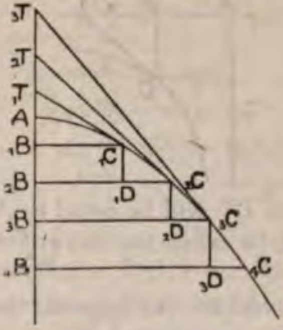
As far as the actual invention of the calculus as he understood the term is concerned, Leibniz received no help from Newton or Barrow; but for the ideas which underlay it, he obtained from Barrow a very great deal more than he acknowledged, and a very great deal less than he would like to have got, or in fact would have got if only he had been more fond of the geometry that he disliked. For, although the Leibnizian calculus was at the time of this essay far superior to that of Barrow on the question of useful application, it was far inferior in the matter of completeness.

(No date.)

Elementa calculi novi pro differentiis et summis, tangentibus et quadraturis, maximis et minimis, dimensionibus linearum, superficierum, solidorum, aliisque communem calculum transcendentibus.

[The elements of the new calculus for differences and sums, tangents and quadratures, maxima and minima, dimensions of lines, surfaces, and solids, and for other things that transcend other means of calculation.]

Let CC be a line, of which the axis is AB, and let BC be ordinates perpendicular to this axis, these being called y, and let AB be the abscissae cut off along the axis, these being called x.



Then CD, the differences of the abscissae, will be called dx; such are ${}_1C_1D$, ${}_2C_2D$, ${}_3C_3D$, etc. Also the straight lines ${}_1D_2C$, ${}_2D_3C$, ${}_3D_4C$, the differences of the ordinates, will be called dy. If now these dx and dy are taken to be infinitely small, or the two points on the curve are understood to be at a distance apart that is less than any given length, i. e., if ${}_1D_2C$, ${}_2D_3C$, etc. are considered as the momentaneous increments[68] of the line BC, increasing continuously as it descends along AB, then it is plain that the straight line joining these two points, ${}_2C_1C$ say, (which is an element of the curve or a side of the infinite-angled polygon that stands for the curve), when produced to meet the axis in ${}_1T$, will be the tangent to the curve, and ${}_1T_1B$ (the interval between the ordinate and the tangent, taken along the axis) will be to the ordinate ${}_1B_1C$ as ${}_1C_1D$ is to ${}_1D_2C$; or, if ${}_1T_1B$ or ${}_2T_2B$, etc. are in general called t, then $t : y :: dx : dy$. Thus to find the differences of series is to find tangents.

For example, it is required to find the tangent to the hyperbola. Here, since $y = \frac{aa}{x}$, supposing that in the diagram, x stands for AB the abscissa along an asymptote, and a for the side of the power, or of the area of the rectangle AB.BC; then

$$dy = -\frac{aa}{xx}dx,$$

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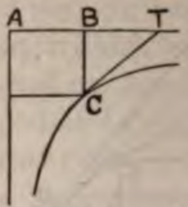
[68] Leibniz has evidently seen Newton's work at the time of this composition; also the use of the word "descends" in the next line again suggests Barrow, while the figure is exactly like the top half of the diagram given by Barrow for Lect. XI, 10, which is the theorem of Gregory that is quoted by Leibniz also. For this figure, see the note to that passage.

as will be soon seen when we set forth the method of this calculus; hence $dx : dy$ or $t : y :: -xx : aa :: -x : \frac{aa}{x} :: -x : y$; therefore $t = -y$,



that is, in the hyperbola BT will be equal to AB, but on account of the sign $-x$, BT must be taken not toward A but in the opposite direction.

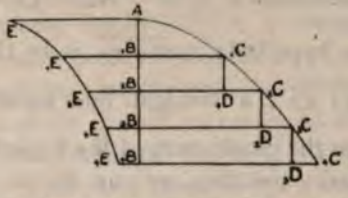
Moreover, differences are the opposite to sums; thus ${}_4B\,{}_4C$ is the sum of all the differences such as ${}_3D\,{}_4C$, ${}_2D\,{}_3C$, etc. as far as A, even if they are infinite in number. This fact I represent thus, $\int dy = y$. Also I represent the area of a figure by the sum of all the rectangles contained by the ordinates and the differences of the abscissae, i. e., by the sum ${}_1B\,{}_1D + {}_2B\,{}_2D + {}_3B\,{}_3D +$ etc. For the narrow triangles ${}_1C\,{}_1D\,{}_2C$, ${}_2C\,{}_2D\,{}_3C$, etc., since they are infinitely small compared with the said rectangles, may be omitted without risk; and thus I represent in my calculus the area of the figure by $\int y\,dx$, or the sum of the rectangles contained by each y and the dx that corresponds to it; here, if the dx's are taken equal to one another, the method of Cavalieri is obtained.

But we, now mounting to greater heights, obtain the area of a figure by finding the figure of its summatrix or quadratrix; and of this indeed the ordinates are to the ordinates of the given figure in the ratio of sums to differences; for instance, let the curve of the figure required to be squared be EE, and let the ordinates to it, EB, which we will call e, be proportional to the differences of the ordinates BC, or to dy; that is let ${}_1B\,{}_1E : {}_2B\,{}_2E :: {}_1D\,{}_2C : {}_2D\,{}_3C$, and so on; or again, let $A\,{}_1B : {}_1B\,{}_1C$, ${}_1C\,{}_1D : {}_1D\,{}_2C$, etc., or $dx : dy$ be in the ratio of a constant or never-varying straight line a to ${}_1B\,{}_1E$ or e; then we have

$$dx : dy :: a : e, \text{ or } e\,dx = a\,dy;$$
$$\therefore \int e\,dx = \int a\,dy.$$

But $e\,dx$ is the same as e multiplied by its corresponding dx, such as the rectangle ${}_3B\,{}_4E$, which is formed from ${}_3B\,{}_3E$ and ${}_3B\,{}_4B$; hence, $\int e\,dx$ is the sum of all such rectangles, ${}_3B\,{}_4E + {}_2B\,{}_1E + {}_3B\,{}_2E$ + etc., and this sum is the figure $A\,{}_4B\,{}_4EA$, if it is supposed that the

dx's, or the intervals between the ordinates e, or BC, are infinitely small. Again, $a\,dy$ is the rectangle contained by a and dy, such as is contained by $_3D\,_4C$ and the constant length a, and the sum of



these rectangles, namely $\int a\,dy$, or $_3D\,_4C.a+{}_2D\,_3C.a+{}_1D\,_2C.a+$etc. is the same as $_3D\,_4C+{}_2D\,_3C+{}_1D\,_2C+$etc. into a, that is, the same as $_4B\,_4C.a$; therefore we have $\int a\,dy=a\int dy=ay$. Therefore $\int e\,dx=ay$, that is, the area $A\,_4B\,_4EA$ will be equal to the rectangle contained by $_4B\,_4C$ and the constant line a, and generally ABEA is equal to the rectangle contained by BC and a.[69]

Thus, for quadratures it is only necessary, being given the line EE, to find the summatrix line CC, and this indeed can always be found by calculus, whether such a line is treated in ordinary geometry or whether it is transcendent and cannot be expressed by algebraical calculation; of this matter in another place.

Now the triangle for the line I call the characteristic of the line, because by its most powerful aid there can be found theorems about the line which are seen to be admirable, such as its length, the surface and solid produced by its rotation, and its center of gravity; for $_1C\,_2C$ is equal to $\sqrt{dx.dx+dy.dy}$. From this we have

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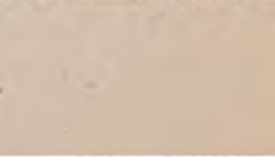
[69] Leibniz does not give a diagram, but it is not difficult to construct his figure from the enunciation that he gives for it. The whole of this paragraph should be compared with the following extract from Barrow (Lect. XI, 19), piece by piece.

"Again, let AMB be a curve of which the axis is AD and let BD be perpendicular to AD; also let KZL be another line such that, when any point M is taken in the curve AB, and through it are drawn MT a tangent to the curve AB, and MFZ parallel to DB, cutting KZ in Z and AD in F, and R is a line of given length, TF : FM = R : FZ. Then the space ADLK is equal to the rectangle contained by R and DB.

For, if DH = R and the rectangle BDHI is completed, and MN is taken to be an indefinitely small arc of the curve AB, and MEX, NOS are drawn parallel to AD; then we have NO : MO = TF : FM = R : FZ;

NO.FZ = MO.R and FG.FZ = ES.EX.



Hence, since the sum of such rectangles as FG.FZ differs only in the least degree from the space ADLK, and the rectangles ES.EX form the rectangle DHIB, the theorem is quite obvious.

at once a method for finding the length of a curve by means of some quadrature; e. g., in the case of the parabola, if $y=\frac{xx}{2a}$, then we have $dy=\frac{xdx}{a}$, and hence $_1C\,_2C=\frac{dx}{a}\sqrt{aa+xx}$; hence, $_1C\,_2C:dx$ as the ordinate of the hyperbola $\sqrt{aa+xx}$ is to the constant line a; that is, $\frac{1}{a}\int dx\sqrt{aa+xx}$, a straight line equal to the arc of a parabola, depends on the quadrature of the hyperbola, as has already been found by others; and thus we can derive by the calculus all the most beautiful results discovered by Huygens, Wallis, van Huraet, and Neil.[70]

I said above that $t:y::dx:dy$; hence we have $t\,dy=y\,dx$, and therefore $\int t\,dy=\int y\,dx$. This equation, enunciated geometrically, gives an elegant theorem due to Gregory,[71] namely that, if BAF is a right angle, and AF = BG, and FG is parallel to AB and equal to BT, that is, $_1F\,_1G={}_1B\,_1T$, then $\int t\,dy$, or the sum of the rectangles contained by t (e.g., $_4F\,_4G$ or $_4B\,_4T$) and dy ($_3F\,_4F$ or $_3D\,_4C$) is equal to the rectangles $_4F\,_3G+{}_3F\,_2G+{}_2F\,_1G+$ etc., or the area of the

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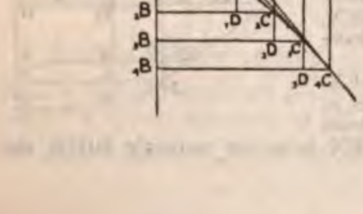
[70] All the things given are to be found in Barrow, but his name is not even mentioned.

[71] This is the strangest coincidence of all! For, Barrow also quotes this very same theorem of Gregory, and no other theorem; also it occurs in this very same Lect. XI that has been referred to already! *Leibniz does not give a diagram; nor from his enunciation could I complete the figure required, until I had referred to the figure given by Barrow*!!! The two diagrams are given below for comparison, Barrow's figure being the one referred to in the note above. Query, is Leibniz's figure taken from Gregory's original, which I have not been able to see, or is it the Leibnizian variation of Barrow's?



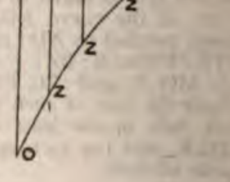


figure A$_4$F$_4$GA is equal to $\int y\,dx$, that is, to the figure A$_4$B$_4$CA; or generally, the figure AFGA is equal to the figure ABCA.

Again, other things, which are immediately evident on inspection, from a figure, are readily deduced by the calculus; for instance, in the case of the trilinear figure ABCA, the figure ABCA together with its complementary figure AFCA is equal to the rectangle ABCF, for the calculus readily shows that $\int y\,dx + \int x\,dy = xy$.

If it is required to find the volume of the solid formed by rotation round an axis, it is only necessary to find $\int y^2\,dx$; for the solid formed by a rotation round the base, $\int x^2 dy$; for the moment about the vertex, $\int yx\,dx$; and these things serve to find the center of gravity of a figure, and also give the frusta of Gregory St. Vincent, and all that Pascal, Wallis, De Laloubère, and others have found out about these matters.

For, if it is required to find the centers of lines, or the surfaces generated by their rotation, e. g., the surface generated by the rotation of the line AC about AB, it is only necessary to find

$$\int \overline{y \sqrt{dx.\,dx + dy.\,dy}}$$

or the sum of every PC applied to the axis at the point B that corresponds to it, (thus $_2$P$_2$C will be applied perpendicular to the axis AB at $_2$B), producing in this way a figure of which the above represents the area. Thus the whole thing will immediately reduce to the quadrature of some plane figure, if, instead of y and dy, their values, obtained from the nature of the ordinates and the tangents to the curve, are substituted. Thus, in the case of the parabola, if y is equal to $\sqrt{2ax}$, then $dy = \dfrac{adx}{y}$ (as will be seen directly); hence we get

$$\int \overline{y\sqrt{dxdx + \frac{aa}{yy}\,dxdx}} \text{ or } \int \overline{dx\sqrt{yy + aa}} \text{ or } \int \overline{dx\sqrt{2ax + aa}},$$

which depends on the quadrature of the parabola (for every $\sqrt{2ax + aa}$ or PC can be applied to a parabola, if it is supposed that AC is the parabola, and AB its axis, provided in that case the figure is changed and the curve turns its concavity toward the axis);[72] and this may be obtained by ordinary geometry, and therefore also a circle will be found equal to the surface of the parabolic conoid; but this is not the place to deduce it at full length.

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[72] The Latin here is rather ambiguous; query, a misprint. But I think I have correctly rendered the argument. It is to be noted that the parabola was at this period always thought of in the form we should now denote by the equation $y = x^2$, and the figure referred to by Leibniz is that which Wallis calls the complement of the semiparabola.

Now these, which may seem to be great matters, are only the very simplest results to be obtained by this calculus; for many much more important consequences follow from it, nor does there occur any simple problem in geometry, either pure or applied to mechanics, that can altogether evade its power. Now we will expound the elements of the calculus itself.

The fundamental principle of the calculus.

Differences and sums are the inverses of one another, that is to say, the sum of the differences of a series is a term of the series, and the difference of the sums of a series is a term of the series; and I enunciate the former thus, $\int dx = x$, and the latter thus, $d\int x = x$.

Thus, let the differences of a series, the series itself, and the sums of the series, be, let us say,

Diffs.		1	2	3	4	5	....	dx
Series	0	1	3	6	10	15	....	x
Sums		0	1	4	10	20	25 ..	$\int x$

Then the terms of the series are the sums of the differences, or $x = \int dx$; thus, $3 = 1 + 2$, $6 = 1 + 2 + 3$, etc.; on the other hand, the differences of the sums of the series are terms of the series, or $d\int x = x$; thus, 3 is the difference between 1 and 4, 6 between 4 and 10.

Also $da = 0$, if it is given that a is a constant quantity, since $a - a = 0$.

Addition and Subtraction.

The difference or sum of a series, of which the general term is made up of the general terms of other series by addition or subtraction, is made up in exactly the same manner from the differences or sums of these series; or

$$x + y - v = \int \overline{dx + dy - dv}, \quad \int \overline{x + y - v} = \int x + \int y - \int v.$$

This is evident at sight, if you take any three series, set out their sums and their differences, and take them together correspondingly as above.

Simple Multiplication.

Here $dxy = xdx + ydy$, or $xy = \int xdx + \int ydy$.

This is what we said above about figures taken together with their complements being equal to the circumscribed rectangle. It is demonstrated by the calculus as follows:

dxy is the same thing as the difference between two successive xy's; let one of these be xy, and the other $x+dx$ into $y+dy$; then we have

$$dxy = \overline{\overline{x+dx}\,.\,\overline{y+dy}} - xy = x\,dy + y\,dx + dx\,dy;$$

the omission of the quantity $dx\,dy$, which is infinitely small in comparison with the rest, for it is supposed that dx and dy are infinitely small (because the lines are understood to be continuously increasing or decreasing by very small increments throughout the series of terms), will leave $x\,dy + y\,dx$; the signs vary according as y and x increase together, or one increases as the other decreases; this point must be noted.

Simple Division.

Here we have $d\dfrac{y}{x} = \dfrac{x\,dy - y\,dx}{xx}$.

For, $d\dfrac{y}{x} = \dfrac{y + dy}{x + dx} - \dfrac{y}{x} = \dfrac{x\,dy - y\,dx}{xx + x\,dx}$, which becomes (if we write xx for $xx + x\,dx$, since $x\,dx$ can be omitted as being infinitely small in comparison with xx) equal to $\dfrac{x\,dy - y\,dx}{xx}$; also, if $y = aa$, then $dy = 0$, and the result becomes $-\dfrac{aadx}{xx}$, which is the value we used a little while before in the case of the tangent to the hyperbola.

From this any one can deduce by the calculus the rules for *Compound Multiplication and Division*; thus,

$$dxvy = xy\,dv + xv\,dy + yv\,dx,$$

$$d\frac{y}{vz} = \frac{xv\,dy - yv\,dz - yz\,dv}{vv.zz};$$

as can be proved from what has gone before; for we have

$$d\frac{y}{x} = \frac{x\,dy - y\,dx}{xx};$$

hence, putting zv for x, and $z\,dv + v\,dz$ for dx or dzv in the above, we obtain what was stated.

Powers follow: $dx^2=2x\,dx$, $dx^3=3x^2\,dx$, and so on. For, putting $y=x$, and $v=x$, we can write dx^2 for dxy, and this is (from above) equal to $x\,dy+y\,dx$, or (if $x=y$, and consequently $dx=dy$) equal to $2x\,dx$. Similarly, for dx^3 we write $dxyv$, that is (from above) $xy\,dv+xv\,dy+yv\,dx$, or (putting x for y and v and dx for dy and dv) equal to $3x^2dx$. Q. E. D. By the same method, in general, $dx^e=e.x^{\underline{e-1}}\,dx$, as can easily be proved from what has been said.

Hence also, $$d\frac{1}{x^h} = -\frac{h\,dx}{x^{\underline{h+1}}}.$$

For, if $\frac{1}{x^h} = x^e$, then $e=-h$, and $x^{e-1}=\frac{1}{x^{\underline{h+1}}}$, as is well known to any one who understands the nature of the exponents in a geometrical progression. The same thing will do for *fractions*. The procedure is the same for irrationals or *Roots*. $d,\sqrt[r]{x^h}=dx^{h:r}$, (where by $h:r$ I mean h/r, or h divided by r), or dx^e (taking e equal to h/r), or $e.x^{\underline{e-1}}\,dx$, by what has been said above, or (by substituting once more $h:r$ for e, and $\overline{h-r}:r$ for $e-1$) $\frac{h}{r}.x^{\overline{h-r:r}}.dx$; and thus finally we get the value of $d,\sqrt[r]{x^h}$.

Moreover, conversely, we have
$$\int ,x^e dx=\frac{x^{\underline{e+1}}}{e+1}, \int\frac{1}{x^e}dx=-\frac{1}{\overline{e-1}.x^{\underline{e-1}}}, \int ,\sqrt[r]{x^h}.dx=\frac{r}{r+h}\sqrt[r]{,x^{\underline{h+r}}}.$$

These are the elementary principles of the differential and summatory calculus, by means of which highly complicated formulas can be dealt with, not only for a fraction or an irrational quantity, or anything else; but also an indefinite quantity, such as x or y, or any other thing expressing generally the terms of any series, may enter into it.

§ XVII.

The next manuscript bears no date; but this can be easily assigned to a certain extent, from internal evidence. It is for one thing later than the publication in the *Acta Eruditorum* of Leibniz's first communication to the world of his calculus in 1684. The manuscript is an answer, or rather the first rough draft probably of such an answer, to the animadversions of Bernhard Nieuwentijt against the idea of the infinitesimal calculus. The latter stated that (i) Leibniz could explain no more than Barrow or

Newton how the infinitely small differences differed from absolute zero; (ii) it was not clear how the differentials of higher order were obtained from those of the first order; (iii) the differential method cannot be applied to exponential functions. Leibniz answers the first point skilfully, fails over the second through erroneous work, which I think he afterward perceived; for he has a note that the whole thing is to be carefully revised before publication. It almost seems that he was not quite confident in his own powers of completely answering these objections, for he also notes that the rudeness of language in which the answer is commenced must be mollified.

On the third point he is silent; in the later written *Historia,* we have seen he is able to get, not over, but round the difficulty of the exponential function; but the silence here would seem to say that Leibniz could not manage exponentials as yet.

The success of the answer to the first point is due to the underlying principle that the ratio $dy : dx$ ultimately becomes a *rate*; when this idea is muddled by an admixture of the infinitesimal idea in the last paragraph the result is almost disastrous. Leibniz, however, looked on his calculus as a tried tool more than anything else.

When my infinitesimal calculus, which includes the calculus of differences and sums, had appeared and spread, certain over-precise veterans began to make trouble; just as once long ago the Sceptics opposed the Dogmatics, as is seen from the work of Empicurus against the mathematicians (i. e., the dogmatics), and such as Francisco Sanchez, the author of the book *Quod nihil scitur,* brought against Clavius; and his opponents to Cavalieri, and Thomas Hobbes to all geometers, and just lately such objections as are made against the quadrature of the parabola by Archimedes by that renowned man, Dethlevus Cluver. When then our method of infinitesimals, which had become known by the name of the calculus of differences, began to be spread abroad by several examples of its use, both of my own and also of the famous brothers Bernoulli, and more especially by the elegant writings of that illustrious Frenchman, the Marquis d'Hopital, just lately a certain erudite mathematician, writing under an assumed name in the scientific *Journal de Trevoux*, appeared to find fault with this method. But to mention one of them by name, even before this there arose against me in Holland Bernard Nieuwentiit, one indeed really well equipped both in learning and ability, but one who wished rather to become known by revising our methods to some extent than by advancing them. Since I introduced not only the first differences, but also the second, third and other higher differences, inassignable or incomparable with these first differences, he wished to appear satisfied with the first only; not considering that the same difficulties existed in the first as in the others that followed, nor that wherever they might be overcome in the first, they also ceased to appear in the rest. Not to mention how a very learned young man, Hermann of Basel, showed that the second and higher differences were avoided by the former in name only, and not in reality; moreover, in demonstrating theorems by the legitimate use of the first differences, by adhering to which he might have accomplished some useful work on his own account, he fails to do so, being driven to fall back on assumptions that are admitted by no one: such as that something different is obtained by multiplying 2 by m and by multiplying m by 2: that the latter was impossible in any case in which the former was possible; also that the square or cube of a quantity is not a quantity or Zero.

In it, however, there is something that is worthy of all praise, in that he desires that the differential calculus should be strengthened with demonstrations, so that it may satisfy the rigorists; and this work he would have procured from me already, and more willingly, if, from the fault-finding everywhere interspersed, the wish had not appeared foreign to the manner of those who desire the truth rather than fame and a name.

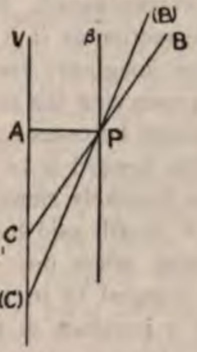
It has been proposed to me several times to confirm the essentials of our calculus by demonstrations, and here I have indicated below its fundamental principles, with the intent that any one who has the leisure may complete the work. Yet I have not seen up to the present any one who would do it. For what the learned Hermann has begun in his writings, published in my defence against Nieuwentiit, is not yet complete.

For I have, beside the mathematical infinitesimal calculus, a method also for use in Physics, of which an example was given in the *Nouvelles de la République des Lettres*; and both of these I include under the Law of Continuity; and adhering to this, I have shown that the rules of the renowned philosophers Descartes and Malebranche were sufficient in themselves to attack all problems on Motion.

I take for granted the following postulate:

In any supposed transition, ending in any terminus, it is permissible to institute a general reasoning, in which the final terminus may also be included.

For example, if A and B are any two quantities, of which the former is the greater and the latter is the less, and while B remains the same, it is supposed that A is continually diminished, until A becomes equal to B; then it will be permissible to include under a general reasoning the prior cases in which A was greater than B, and also the ultimate case in which the difference vanishes and A is equal to B. Similarly, if two bodies are in motion at the same time, and it is assumed that while the motion of B remains the same, the velocity of A is continually diminished until it vanishes altogether, or the speed of A becomes zero; it will be permissible to include this case with the case of the motion of B under one general reasoning. We do the same thing in geometry, when two straight lines are taken, produced in any manner, one VA being given in position or remaining in the same site, the other BP passing through a given point P, and varying in position while the point P remains fixed; at first indeed converging toward the line VA and meeting it in the point C; then, as the angle of inclination VCA is continually diminished, meeting VA in some more remote point (C), until at length from BP, through the position (B)P, it comes



to βP, in which the straight line no longer converges toward VA, but is parallel to it, and C is an impossible or imaginary point. With this supposition it is permissible to include under some one general reasoning not only all the intermediate cases such as (B)P but also the ultimate case βP.

Hence also it comes to pass that we include as one case ellipses and the parabola, just as if A is considered to be one focus of an ellipse (of which V is the given vertex), and this focus remains fixed, while the other focus is variable as we pass from ellipse to ellipse, until at length (in the case when the line BP, by its intersection with the line VA, gives the variable focus) the focus C becomes evanescent[73] or impossible, in which case the ellipse passes into a parabola. Hence it is permissible with our postulate that a parabola should be considered with ellipses under a common reasoning. Just as it is common practice to make use of this method in geometrical constructions, when they include under one general construction many different cases, noting that in a certain case the converging straight line passes into a parallel straight line, the angle between it and another straight line vanishing.

Moreover, from this postulate arise certain expressions which are generally used for the sake of convenience, but seem to contain an absurdity, although it is one that causes no hindrance, when its proper meaning is substituted. For instance, we speak of an imaginary point of intersection as if it were a real point, in the same manner as in algebra imaginary roots are considered as accepted numbers. Hence, preserving the analogy, we say that, when the straight line BP ultimately becomes parallel to the straight line VA, even then it converges toward it or makes an angle with it, only that the angle is then infinitely small; similarly, when a body ultimately comes to rest, it is still said to have a velocity, but one that is infinitely small; and, when one straight line is equal to another, it is said to be unequal to it, but that the difference is infinitely small; and that a parabola is the ultimate form of an ellipse, in which the second focus is at an infinite distance from the given focus nearest to the given vertex, or in which the ratio of PA to AC, or the angle BCA, is infinitely small.

Of course it is really true that things which are absolutely equal have a difference which is absolutely nothing; and that straight lines which are parallel never meet, since the distance between them is everywhere the same exactly; that a parabola is not an ellipse at all, and so on. Yet, a state of transition may be imagined, or one of evanescence, in which indeed there has not yet arisen exact equality or rest or parallelism, but in which it is passing into such a state, that the difference is less than any assignable quantity; also that in this state there will still remain some difference, some velocity, some angle, but in each case one that is infinitely small; and the distance of the point of intersection, or the variable focus, from the fixed focus will be infinitely great, and the parabola may be included under the heading of an ellipse (and also in the some manner and by the same reasoning under the heading of a hyperbola), seeing that those things that are found to be true about a parabola of this kind are in no way different, for any construction, from those which can be stated by treating the parabola rigorously.

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[73] The term is here used with the idea of "vanishing into the far distance."

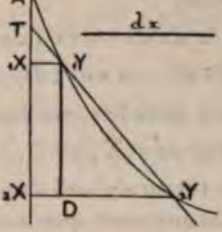
Truly it is very likely that Archimedes, and one who seems so have surpassed him, Conon, found out their wonderfully elegant theorems by the help of such ideas; these theorems they completed with *reductio ad absurdum* proofs, by which they at the same time provided rigorous demonstrations and also concealed their methods. Descartes very appropriately remarked in one of his writings that Archimedes used as it were a kind of metaphysical reasoning (Caramuel would call it metageometry), the method being scarcely used by any of the ancients (except those who dealt with quadratrices); in our time Cavalieri has revived the method of Archimedes, and afforded an opportunity for others to advance still further. Indeed Descartes himself did so, since at one time he imagined a circle to be a regular polygon with an infinite number of sides, and used the same idea in treating the cycloid; and Huygens too, in his work on the pendulum, since he was accustomed to confirm his theorems by rigorous demonstrations; yet at other times, in order to avoid too great prolixity, he made use of infinitesimals; as also quite lately did the renowned La Hire.

For the present, whether such a state of instantaneous transition from inequality to equality, from motion to rest, from convergence to parallelism, or anything of the sort, can be sustained in a rigorous or metaphysical sense, or whether infinite extensions successively greater and greater, or infinitely small ones successively less and less, are legitimate considerations, is a matter that I own to be possibly open to question; but for him who would discuss these matters, it is not necessary to fall back upon metaphysical controversies, such as the composition of the continuum, or to make geometrical matters depend thereon. Of course, there is no doubt that a line may be considered to be unlimited in any manner, and that, if it is unlimited on one side only, there can be added to it something that is limited on both sides. But whether a straight line of this kind is to be considered as one whole that can be referred to computation, or whether it can be allocated among quantities which may be used in reckoning, is quite another question that need not be discussed at this point.

It will be sufficient if, when we speak of infinitely great (or more strictly unlimited), or of infinitely small quantities (i. e., the very least of those within our knowledge), it is understood that we mean quantities that are indefinitely great or indefinitely small, i. e., as great as you please, or as small as you please, so that the error that any one may assign may be less than a certain assigned quantity. Also, since in general it will appear that, when any small error is assigned, it can be shown that it should be less, it follows that the error is absolutely nothing; an almost exactly similar kind of argument is used in different places by Euclid, Theodosius and others; and this seemed to them to be a wonderful thing, although it could not be denied that it was perfectly true that, from the very thing that was assumed as an error, it could be inferred that the error was non-existent. Thus, by infinitely great and infinitely small, we understand something indefinitely great, or something indefinitely small, so that each conducts itself as a sort of class, and not merely as the last thing of a class. If any one wishes to understand these as the ultimate things, or as truly infinite, it can be done, and that too without falling back upon a controversy about the reality of extensions, or of infinite continuums in general, or of the infinitely small, ay, even though he think that such things are utterly impossible; it will be sufficient simply to make use of them as a tool that has advantages for the purpose of the calculation, just as the algebraists retain imaginary roots with great profit. For they contain a handy means of reckoning, as can manifestly be verified in every case in a rigorous manner by the method already stated.

But it seems right to show this a little more clearly, in order that it may be confirmed that the algorithm, as it is called, of our differential calculus, set forth by me in the year 1684, is quite reasonable. First of all, the sense in which the phrase "*dy* is the element of y," is to be taken will best be understood by considering a line AY referred to a straight line AX as axis.

Let the curve AY be a parabola, and let the tangent at the vertex A be taken as the axis. If AX is called x, and AY, y, and the latus-rectum is a, the equation to the parabola will be $xx=ay$, and this holds good at every point. Now, let $A\,{}_1X=x$, and ${}_1X\,{}_1Y=y$



and from the point ${}_1Y$ let fall a perpendicular ${}_1YD$ to some greater ordinate ${}_2X\,{}_2Y$ that follows, and let ${}_1X\,{}_2X$, the difference between $A\,{}_1X$ and $A\,{}_2X$, be called dx; and similarly, let $D\,{}_2Y$, the difference between ${}_1X\,{}_1Y$ and ${}_2X\,{}_2Y$, be called dy.

Then, since $y=xx:a$, by the same law, we have

$$y+dy=xx+2x\,dx+dx\,dx,:a;$$

and taking away the y from the one side and the $xx:a$ from the other, we have left

$$dy:dx=2x+dx:a;$$

and this is a general rule, expressing the ratio of the difference of the ordinates to the difference of the abscissae, or, if the chord ${}_1Y\,{}_2Y$ is produced until it meets the axis in T, then the ratio of the ordinate ${}_1X\,{}_1Y$ to $T\,{}_1X$, the part of the axis intercepted between the point of intersection and the ordinate, will be as $2x+dx$ to a. Now, since by our postulate it is permissible to include under the one general reasoning the case also in which the ordinate ${}_2X\,{}_2Y$ is moved up nearer and nearer to the fixed ordinate ${}_1X\,{}_1Y$ until it ultimately coincides with it, it is evident that in this case dx becomes equal to zero and should be neglected, and thus it is clear that, since in this case $T\,{}_1Y$ is the tangent, ${}_1X\,{}_1Y$ is to $T\,{}_1X$ as $2x$ is to a.

Hence, it may be seen that there is no need in the whole of our differential calculus to say that those things are equal which have a difference that is infinitely small, but that those things can be taken as equal that have not any difference at all, provided that the calculation is supposed to be general, including both the cases in which there is a difference and in which the difference is zero;

and provided that the difference is not assumed to be zero until the calculation is purged as far as is possible by legitimate omissions, and reduced to ratios of non-evanescent quantities, and we finally come to the point where we apply our result to the ultimate case.

Similarly, if $x^3 = aay$, then we have

$$x^3 + 3xx\,dx + 3x\,dx\,dx + dx\,dx\,dx = aay + aa\,dy,$$

or cancelling from each side,

$$3xx\,dx + 3x\,dx\,dx + dx\,dx\,dx = aa\,dy,$$

or $$3xx + 3x\,dx + dx\,dx,: aa = dy : dx = {}_1X\,{}_1Y : T\,{}_1X;$$

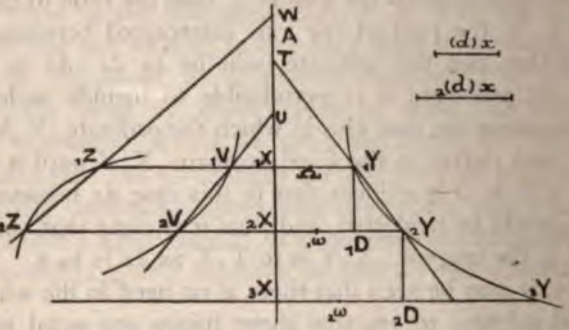
hence, when the difference vanishes, we have

$$3xx : aa = {}_1X\,{}_1Y : T\,{}_1X.$$

But if it is desired to retain *dy* and *dx* in the calculation, so that they may represent non-evanescent quantities even in the ultimate case, let any assignable straight line be taken as (*dx*), and let the straight line which bears to (*dx*) the ratio of y or ${}_1X\,{}_1Y$ to ${}_1XT$ be called (*dy*); in this way *dy* and *dx* will always be assignables bearing to one another the ratio of $D\,{}_2Y$ to $D\,{}_1Y$, which latter vanish in the ultimate case.

[Leibniz here gives a correction for a passage in the *Acta Eruditorum,* which is unintelligible without the context.]

On these suppositions, all the rules of our algorithm, as set out in the *Acta Eruditorum* for October 1684, can be proved without much trouble.



Let the curves YY, VV, ZZ be referred to the same axis AXX; and to the abscissae $A\,{}_1X$ $(=x)$ and $A\,{}_2X$ $(=x+dx)$ let there correspond the ordinates ${}_1X\,{}_1Y$ $(=y)$ and ${}_2X\,{}_2Y$ $(=y+dy)$, and also the ordinates ${}_1X\,{}_1V$ $(=v)$ and ${}_2X\,{}_2V$ $(=v+dv)$, and the ordinates

${}_1X\,{}_1Z\ (=z)$ and ${}_2X\,{}_2Z\ (=z+dz)$. Let the chords ${}_1Y\,{}_2Y$, ${}_1V\,{}_2V$, ${}_1Z\,{}_2Z$, when produced meet the axis AXX in T, U, W. Take any straight line you will as $(d)x$, and, while the point ${}_1X$ remains fixed and the point ${}_2X$ approaches ${}_1X$ in any manner, let this remain constant, and let $(d)y$ be another line which bears to $(d)x$ the ratio of y to ${}_1XT$, or of dy to dx; and similarly, let $(d)v$ be to $(d)x$ as v to ${}_1XU$ or dv to dx; also let $(d)z$ be to $(d)x$ as z to ${}_1XW$ or dz to dx; then $(d)x$, $(d)y$, $(d)z$, $(d)w$ will always be ordinary or assignable straight lines.

Nor for *Addition and Subtraction* we have the following:

$$\text{If } y-z=v, \text{ then } (d)y-(d)z=(d)v.$$

This I prove thus: $y+dy-z-dz=v+dv$, (if we suppose that as y increases, z and v also increase; otherwise for decreasing quantities, for z say, $-dz$ should be taken instead of dz, as I mentioned once before); hence, rejecting the equals, namely $y-z$ from one side, and v from the other, we have $dy-dz=dv$, and therefore also $dy-dz:dx=dv:dx$. But $dy:dx$, $dz:dx$, $dv:dx$ are respectively equal to $(d)y:(d)x$, $(d)z:(d)x$, and $(d)v:(d)x$. Similarly, $(d)z:(d)y$ and $(d)v:(d)y$ are respectively equal to $dz:dy$ and $dv:dy$. Hence, $(d)y-(d)z, :(d)x=(d)v:(d)x$; and thus $(d)y-(d)z$ is equal to $(d)v$, which was to be proved; or we may write the result as $(d)v:(d)y=1-(d)z:(d)y$.

This rule for addition and subtraction also comes out by the use of our postulate of a common calculation, when ${}_1X$ coincides with ${}_2X$, and ${}_1YT$, ${}_1YU$, ${}_1YW$ are the tangents to the curves YY, VV, ZZ. Moreover, although we may be content with the assignable quantities $(d)y$, $(d)v$, $(d)z$, $(d)x$, etc., since in this way we may perceive the whole fruit of our calculus, namely a construction by means of assignable quantities, yet it is plain from what I have said that, at least in our minds, the unassignables dx and dy may be substituted for them by a method of supposition even in the case when they are evanescent; for the ratio $dy:dx$ can always be reduced to the ratio $(d)y:(d)x$, a ratio between quantities that are assignable or undoubtedly real. Thus we have in the case of tangents $dv:dy=1-dz:dx$, or $dv=dy-dz$.

Multiplication. Let $ay=xv$, then $a(d)y=x(d)v+v(d)x$.

Proof. $ay+a\,dy=x+dx$, $v+dv=xv+x\,dv+v\,dx+dx\,dv$;

and, rejecting the equals ay and xy from the two sides,

$$a\,dy = x dv + v\,dx + dx\,dv,$$

or

$$\frac{a\,dy}{dx} = \frac{x\,dv}{dx} + v + dv;$$

and transferring the matter, as we may, to straight lines that never become evanescent, we have

$$\frac{a(d)y}{(d)x} + \frac{x(d)y}{(d)x} + v + dv;$$

so that, since it alone can become evanescent, dv is superfluous, and in the case of the vanishing differences, as in that case $dv=0$, we have

$$a(d)y = x(d)v + v(d)x, \text{ as was stated,}$$

or $$(d)y : (d)x = x + v,: a.$$

Also, since $(d)y : (d)x$ always $= dy : dx$, it will be allowable to suppose this is true in the case when dy, dx become evanescent, and to say that $dy : dx = x + v : a$, or $a\,dy = x\,dv + v\,dx$.

Division. Let $z : a = v : x$, then $(d)z : a = v(d)x - x(d)y,: xx$.

Proof $$z + dz : a = v + dv,: ,x + dx;$$

or clearing of fractions, $xz + xdz + zdx + dzdx = av + adv$; taking away the equals xz and av from the two sides, and dividing what is left by dx, we have

$$a\,dv - x\,dz,: dx = z + dz,$$

$$\text{or } a(d)v - x(d)z,: dx = z + dz;$$

and thus, only dz, which can become evanescent, is superfluous. Also, in the case of vanishing differences, when ${}_1X$ coincides with ${}_2X$, since in that case $dz = 0$, we have

$$a(d)v - x(d)z,:(d)x = z = av : x;$$

whence, (as was stated) $(d)z = ax(d)v - av(d)x,: xx$,

or $$(d)z : (d)x = (a : x)(d)v : (d)x - av : xx.$$

Also, since $(d)z : (d)x$ is always equal to $dz : dx$, on all other occasions, it is allowable to suppose this to be so also when dz, dv, dx are evanescent, and to put

$$dz : dx = ax\,dv - av\,dx,: xx$$

For *Powers,* let the equation be $a^{\underset{\cdot}{\underline{n-e}}}x^{e} = y^{n}$, then

$$\frac{(d)\,y}{(d)\,x} = \frac{e..x^{\underset{\cdot}{\underline{e-1}}}}{n.y^{\underset{\cdot}{\underline{n-1}}}};$$

and this I will prove in a manner a little more detailed than those above, thus:

$$a^{n-e},\frac{1}{1}x^{e}+\frac{e}{1}x^{e\div 1}dx+\frac{e,e-1}{1,2}x^{e\div 2}dxdx,+\frac{e,e-1,e-2}{1,2,3}x^{e\div 3}dxdxdx$$

(and so on until the factor $e-e$ or 0 is reached)

$$=\frac{1}{1}y^{n}+\frac{n}{1}y^{n\div 1}dy+\frac{n,n-1}{1,2}y^{n\div 2}dydy+\frac{n,n-1,n-2}{1,2,3}y^{n\div 3}dydydy$$

(and so on until the factor $n-n$ or 0 is reached);
take away from the one side $a^{n\div e}x^{e}$, and from the other side y^{n}, these being equal to one another, and divide what is left by dx, and lastly, instead of the ratio $dy:dx$, between the two quantities that continually diminish, substitute the ratio that is equal to it, $(d)y:(d)x$, a ratio between two quantities, of which one, $(d)x$, always remains the same during the time that the differences are diminishing, or while ${}_2X$ is approaching the fixed point ${}_1X$ and we have

$$\frac{e}{1}x^{e-1}+\frac{e,e-1}{1,2}x^{e\div 2}dx+\frac{e,e-1,e-2}{1,2,3}x^{e\div 3}dxdx+\text{etc.}$$

$$=\frac{n}{1}y^{n\div 1}\frac{(d)y}{(d)x}+\frac{n,n-1}{1,2}y^{n\div 2}\frac{(d)y}{(d)x}dy+\frac{n,n-1,n-2}{1,2,3}y^{n\div 3}\frac{(d)y}{(d)x}dydy+\text{etc.}$$

Now, since by the postulate there is included in this general rule the case also in which the differences become equal to zero, that is when the points ${}_2X$, ${}_2Y$ coincide with the points ${}_1X$, ${}_1Y$ respectively; therefore, in that case, putting dx and dy equal to 0, we have

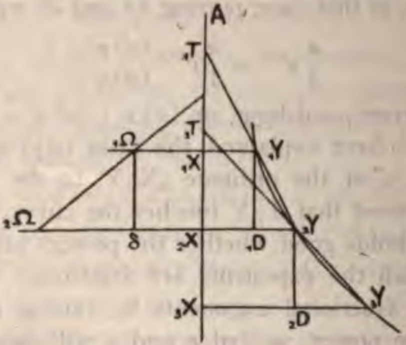
$$\frac{e}{1}x^{e-1}=\frac{n}{1}y^{n-1}\frac{(d)y}{(d)x},$$

the remaining terms vanishing, or $(d)y : (d)x = e.x^{x-1} : n\ y^{n\div 1}$. Moreover, as we have explained, the ratio $(d)y:(d)x$ is the same as the ratio of y, or the ordinate ${}_1X{}_1Y$, to the subtangent ${}_1XT$, where it is supposed that $T{}_1Y$ touches the curve in ${}_1Y$.

This proof holds good whether the powers are integral powers or roots of which the exponents are fractions. Though we may also get rid of fractional exponents by raising each side of the equation to some power, so that e and n will then signify nothing else but powers with rational exponents, and there will be no need of a series proceeding to infinity. Moreover, at any rate, it will be permissible, by means of the explanation given above, to return to the unassignable quantities dy and dx, by making in the case of evanescent differences, as in all other cases, the supposition that the ratio of the evanescent quantities dy and dx is equal to the ratio of $(d)y$ and $(d)x$, because this supposition can always be reduced to an undoubtable truth.

Thus far the algorithm has been demonstrated for differences of the first order: now I will proceed to show that the same method will hold good for the differences of the differences. For this purpose, take three ordinates, ${}_1X_1Y$, ${}_2X_2Y$, ${}_3X_3Y$, of which ${}_1X_1Y$ remains constant, but ${}_2X_2Y$ and ${}_3X_3Y$ continually approach ${}_1X_1Y$ until finally they both coincide with it simultaneously; which will happen if the speed with which ${}_3X$ approaches ${}_1X$ is to the speed with which ${}_2X$ approaches ${}_1X$ is in the ratio of ${}_1X_3X$ to ${}_1X_2X$. Also let two straight lines be assigned, $(d)x$ always constant for any position of ${}_2X$, and ${}_2(d)x$ for any position of ${}_3X$; also let $(d)y$ always be to $(d)x$ as D_2Y is to ${}_1X_2X$, or as y (i. e., ${}_1X_1Y$) is to ${}_1XT$; thus, while $(d)x$ remains always the same, $(d)y$ will be altered as ${}_2X$ approaches ${}_1X$; similarly, let ${}_2(d)y$ be to ${}_2(d)x$ as ${}_2D_3Y$ to ${}_2X_3X$ or as $y+dy$ (i. e., ${}_2X_2Y$) to ${}_2X_2T$; thus while ${}_2(d)x$ remains constant, ${}_2(d)y$ will be altered as ${}_3X$ approaches ${}_1X$.

Also let $(d)y$ be always taken in the varying line ${}_2X_2Y$, and let ${}_2X_1\omega$ be equal to $(d)y$, and similarly take ${}_2(d)y$ in the line ${}_3X_3Y$, and let ${}_3X_2\omega$ be equal to ${}_2(d)y$. Thus, while ${}_2X$ and ${}_3X$ continually approach to the straight line ${}_1X_1Y$, ${}_2X_1\omega$ and ${}_3X_2\omega$ continually approach it also, and finally coincide with it at the same time as ${}_2X$ and ${}_3X$. Further, let the point in the ordinate ${}_1X_1Y$, which ${}_1\omega$ continually approaches and with which it at last coincides, be marked, and let it be Ω; then ${}_1X\Omega$ is the ultimate $(d)y$, which bears to $(d)x$ the ratio of the ordinate ${}_1X_1Y$ to the subtangent ${}_1XT$, where it is supposed that T_1X touches the curve in ${}_1Y$, because then indeed ${}_1Y$ and ${}_2Y$ coincide. Now, since all this can be done,



no matter where ${}_1\mathrm{Y}$ may be taken on the curve, it is evident that a curve $\Omega\Omega$ will be produced in this way, which is the differentrix of the curve YY; just as, conversely, the curve YY is the summatrix curve of $\Omega\Omega$, as can be readily demonstrated.

By this method, the calculus may be demonstrated also for the differences of the differences.

Let ${}_1\mathrm{X}\,{}_1\mathrm{Y}$, ${}_2\mathrm{X}\,{}_2\mathrm{Y}$, ${}_3\mathrm{X}\,{}_3\mathrm{Y}$ be three ordinates, of which the values are y, $y+dy$, $y+dy+ddy$, and let ${}_1\mathrm{X}\,{}_2\mathrm{X}$ (dx) and ${}_2\mathrm{X}\,{}_3\mathrm{X}$ $(dx+ddx)$ be any distances, and $\mathrm{D}\,{}_2\mathrm{Y}$ (dy) and ${}_2\mathrm{D}\,{}_3\mathrm{Y}$ $(dy+ddy)$ the differences. Now the difference between $(d)y$ and ${}_2(d)y$, or between ${}_1\mathrm{X}\Omega$ and ${}_2\mathrm{X}\,{}_2\Omega$ is $\delta\,{}_2\Omega$, and that between ${}_1\mathrm{X}\,{}_2\mathrm{X}$ and ${}_2\mathrm{X}\,{}_3\mathrm{X}$ is ddx; also let

$(d)dx : (d)x = dx : {}_2(d)x$, [74] and similarly let

$(d)dy : (d)y = {}_2\Omega\delta : {}_1\mathrm{X}\,{}_2\mathrm{X}$ or ${}_1\mathrm{X}\Omega : {}_1\mathrm{XT}$.

Now, for the sake of example, let us take $ay = xv$. Then we have $a\,dy = x\,dv + v\,dx + dx\,dv$, as has been shown above; and similarly,

$$\begin{aligned} a\,dy + a\,ddy &= (x+dx)(dv+ddv) + (v+dv)(dx+ddx) \\ &\qquad + (dx+ddx)(dv+ddv) \\ &= x\,dv + x\,ddv + dx\,dv + dx\,ddv + v\,dx + v\,ddx \\ &\qquad + dv\,dx + dv\,ddx + dx\,dv + dx\,ddv \\ &\qquad + ddx\,dv + ddx\,ddv. \end{aligned}$$ [75]

Taking away $a\,dy$ from one side, and $x\,dx + v\,dx + dx\,dv$ from the other, there will be left in any case

$$\frac{ddy}{ddx} = \frac{x}{a}\,\frac{ddy}{ddx} + \frac{v}{a} + \frac{2}{a}\,\frac{dx\,dv}{ddx} + \frac{2\,dv}{a} + \frac{2\,dx\,ddx}{a\,ddx} + \frac{ddv}{a}.$$

In this it is evident that the ratio between ddy and ddx can be expressed by the ratio of the straight line $(d)dy$ to $(d)x$, the straight line assumed above, which we have supposed to remain constant as ${}_2\mathrm{X}$ and ${}_3\mathrm{X}$ approach ${}_1\mathrm{X}$. Also, since $(d)dx$, (since it bears an assignable ratio to $(d)x$, however nearly ${}_2\mathrm{X}$ approaches to ${}_1\mathrm{X}$, or

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[74] This makes $(d)dx$ an inassignable. It may be a misprint due to a slip of Leibniz, or of Gerhardt in transcription; for there is no similarity between it and the statement in the next line. I cannot however offer any feasible suggestion for correction.

[75] This is quite wrong. Leibniz has evidently substituted $x + dx$ for x, etc.; which is not legitimate unless ${}_2\mathrm{X}\,{}_3\mathrm{Y}$ is taken as $y + dy + d(y + dy)$, and so on; even then fresh difficulties would be introduced. As it stands, this line should read

$$a\,dy + a\,ddy = x(dv + ddv) + v(dx + ddx) + (dx + ddx)(dv + ddv).$$

On account of this error and that noted above, there is not much profit in considering the remainder of this passage.

however much dx, the difference between the abscissae, is diminished), is not evanescent, even when, finally, dx and ddx, dv and ddv, are all supposed to be zero. In the same way, the ratio of ddv to ddx may be expressed by the ratio of an assignable straight line $(d)dv$ to the assumed constant $(d)x$; and even the ratio of $dv\,dx$ to $a\,ddx$ may be so expressed; for, since $dv:dx=(d)v:(d)x$, therefore $dv\,dx:dx\,dx=(d)v:(d)x$. Hence, if a new straight line, $(dd)x$, is assumed to be such that $a\,ddx:dx\,dx=(dd)x\text{s}(d)x$, then the new straight line will be assignable, even though dx, ddx, etc. become evanescent. Since therefore $dv\,dx:dx\,dx=(d)v:(d)x$ and $dv\,dx:a\,ddx=(d)x:(\mathrm{dd})x$, it follows that $dv\,dx:a\,ddx=(d)v:(dd)x$, an thus at length there is prouced an equation that is freed as far as possible from those ratios that might become evenescent, namely,

$$\frac{(d)\,dy}{(d)dx}=\frac{x}{a}\frac{(d)\,dy}{(d)dx}+\frac{y}{a}+\frac{2\,(d)y}{(dd)x}+\frac{2\,dv}{a}+\frac{2\,dx}{a}\frac{(d)\,dy}{(d)dx}+\frac{ddv}{a}.$$

Thus far all the straight lines have been considered to be assignable so long as ${}_1$X and ${}_2$X do not coincide; but in the case of coincidence, dv and ddv are zero, and we have

$$\frac{(d)dy}{(d)dx}=\frac{x}{a}\frac{(d)\,dv}{(d)dx}+\frac{v}{a}+\frac{2\,(d)y}{(dd)x}+\frac{0}{a}+\frac{2\,(d)dv}{(d)dx}\frac{0}{a}+\frac{0}{a},$$

or, omitting terms equal to zero,

$$\frac{(d)dy}{(d)dx}=\frac{x}{a}\frac{(d)\,dv}{(d)dx}+\frac{v}{a}+\frac{2\,(d)y}{(dd)x}.$$

Hence, if dx, ddx, dv, ddv, dy, ddy, are by a certain fiction imagined to remain, even when they become evanescent, as if they were infinitely small quantities (and in this there is no danger, since the whole matter can be always referred back to assignable quantities), then we have in the case of coincidence of the point ${}_1$X and ${}_2$X the equation

$$\frac{ddv}{ddx}=\frac{x}{a}\,\frac{ddy}{ddx}+\frac{v}{a}+\frac{2}{a}\,\frac{dx\,dy}{ddx}.$$

J. M. Child.

Derby, England.

LIBRA:

THE ETERNAL BALANCE OF GOOD AND ILL.

I.

FROM everlasting is the Universe,
And unto everlasting shall extend;
Without beginning is it; without end
Its morrows ever yesterdays rehearse;
Not first nor last but only midst it knows;
As never young, so never old it grows.

II.

Yet is the secret of its permanence
Not rest but striving, not a dead repose,
No peace of mutually slaughtered foes,
Nor truce of wearied, but a strife intense,
Deathless, of powers that charge and countercharge
Ever, yet never may their bounds enlarge.

III.

Not progress is the secret of the sky,
And not decay the withering doom of earth;
Though, out of star-mist, systems round to birth,
And a dead moon mirrors earth's destiny,
The star shall sink in darkness whence it came,
And earth's grim desert be reborn in flame.

IV.

It is the wave with endless rise and fall,

It is the tide with ceaseless ebb and flow,
The changing moons, the hours with gloom and glow,
That hold the mystery of each and all,—
The rhythmic secret, wherein man has part
Even from the first pulsation of his heart.

V.

The pendulum with its untiring swing
Not only metes out time, but it reveals,
Babbling, the word eternity conceals,
Though to men deaf with their own questioning;
The lilting ripple of the poet's song
Itself contains the clue he sought life-long.

VI.

Nothing can be unfolded but has first
Been folded in, and shall be so again;
Nor yet can aught in equipoise remain,
But ever driveth toward the best or worst;
Nature keeps neither full nor empty cup,
And the half-filled she drains or fills it up.

VII.

Yet what had no beginning always is
And never can become; no inward change,
However wide its outward motions range,
Can touch its heart; despite man's fantasies,
The Universe exists, not merely seems
An everlasting see-saw of extremes.

VIII.

These two extremes man knows as More and Less,
As Good and Ill, lastly as Right and Wrong;
Feels them as Love or Hate his pulses throng;

Sees them with Beauty clothed or Ugliness,
And names them from their power to bless or ban
God and Devil, Ormuzd and Ahriman.

IX.

The righteous Paul lamented in his heart
The Good by Evil thwarted. So in thine
The False and True, the Cruel and Benign,
The Pure and Impure make thee what thou art
And what the All is: tiger, dove, and man,
Seraph and fiend, are fashioned on one plan.

X.

Even as the Universe, mid seeming change,
Really is locked in iron permanence,
So, everywhere, despite our cheated sense,
From one self-nature may it never range:
One is it, one in body and the soul,
And every part is parcel of the whole.

XI.

Behind all forces hides the primal Force,
The Unconditioned, which is bad and good
Impartially, and its divided mood
The single spirit of the Universe,
Of you and me and all men and the earth
And all the worlds Infinity wheels forth.

XII.

But mortal life displays not one but two,
Shows Good all-perfect warring against Ill,
Which yet abides unconquerable still,
And in this duel sets for man a part,
And teaches he must choose the side of Good,
Or rank below the cleft, insensate wood.

XIII.

Had it been destined to be otherwise,
Long since it would have been so; nay, for we
Deal not with time but with eternity,
It would have been so always; had our skies
Been fated to o'erarch a perfect earth,
They would have overarched it from their birth.

XIV.

This is the revelation; this alone
Rained ever from the Milky Way adown,
Or flamed from Vega and the Northern Crown,
Even this that written on my heart I own.
Not ours to ask if unto me or you
The word be welcome, but if it be true.

XV.

What then must be the Universe, ideal?
Never and nowhere; but endurable,
A place where on the whole 'tis fairly well,
Where at least men can live; in short, the real.
Had it been more, there were no need to ask;
Had it been less, not ours had been the task.

XVI.

If this be true, as Life forbids to doubt,
Is low then one with high, is conscience vain?
Forever no! But, though I shall not gain
After short strife a glorious mustering out,
My privilege more glorious is to be
A soldier of the Right eternally.

XVII.

Yet what avails my battle for the Right,

You ask, if through eternity shall still
Be kept the balance between Good and Ill?
Me much avails it, for 'tis mine to fight
On the Lord's side, being birthmarked with his seal;
My joy, my life is in that battle peal.

XVIII.

More can I ask? Shall some far eon see
The Evil quelled, the Good supreme prevail?
Not if our world have told us a true tale.
But can we hear and judge it rightfully?
Our torch is feeble; but at least its light
Reveals us friend and foeman in the fight.

XIX.

The rest is God's. Yet who would change that could
Doom so divine, which loftiest souls must bear,
Though archangelic?—in all worlds to share
The warfare of the soldiers of the Good,
Though marching under orders ever sealed,
And battling ever on a doubtful field!

HARRY LYMAN KOOPMAN.

PROVIDENCE, R. I.

CRITICISMS AND DISCUSSIONS.

LOGIC AND PSYCHOLOGY.

The nature and purpose of symbolic or mathematical logic, which began to be developed by Leibniz and was continued quite independently by Boole and others, is tolerably well known by now. Logical reasoning is translated by it into what Leibniz called a "real characteristic" which is very analogous to ordinary algebra, and helps swiftness and accuracy of reasoning—even complicated reasoning—in much the same way as the signs in algebra do. This tendency culminated in the very ingenious and useful "mathematical logic" of Peano. Peano's system was far more complete than Boole's, for the whole of a piece of reasoning which included algebraic formulas and equations could be put into a symbolical form in which ordinary words—which are not part of a "real characteristic"—are not used. In this direction Peano's system met the much earlier system devised by Frege. However, Frege's system was not thought out so much with a view to rapidity of reasoning and convenience of writing as with a view to emphasizing slight and important logical distinctions in very similar concepts and deductions and consequently a scrupulous accuracy in deductions. It may thus be noticed, by the way, that the purpose of Frege's symbolism was different from that of all previous symbolisms in logic and mathematics, for Frege wished to lay stress upon the *differences* in various analogous ideas and deductions rather than upon their *analogies.* Broadly speaking, Russell and Whitehead's work may be characterized by saying that it is formed under the influence of a combination of the two tendencies represented by Frege and Peano. The convenient symbolism of Peano is retained wherever possible and the superior analysis and subtlety of Frege is fully used. We ought to add also that nearly all of Frege's discoveries were made independently by Russell himself, Frege's great work having been neglected by philosophers and mathematicians.

There is one great point in which Russell's works differs much from that of Frege: full use is made of the enormously important researches of Georg Cantor on transfinite numbers. While putting on a firm basis the treatment of infinite classes and numbers, Cantor's work led to the recognition of forms of a paradox absolutely fundamental in logic. After many vain attempts by various mathematicians and philosophers, this paradox has been satisfactorily solved by the thorough remoulding of logic given in Whitehead and Russell's *Principia Mathematica.*

From Peano's various *Formulaires* to the work last mentioned the subject-matter is principally the collection of truths which we can reach by logical deduction from logical principles. This body of truths is not a description of psychological methods of discovery or psychological results, but is of course reached by psychical processes, like most other discoveries in a purely intellectual domain. It is then simply irrelevant to complain that there is no place in the *Formulaires* or *Principia* for that "intuition" which brings about mathematical discoveries. It would be just as much to the point to complain that in what is excavated we do not discover the tools used for excavating or the method of excavation. And yet this is what the rather superficial and amusing discussions of Henri Poincaré are mostly about. And these discussions are what Prof. J. B. Shaw in the number of *The Monist* for July, 1916, refers to (p. 397) as Poincaré's "successful attacks on logistic." We might reasonably, it seems to me, have expected that Professor Shaw should make some reference to the reply by Louis Couturat to Poincaré which was translated in *The Monist* for October, 1912, and which is quite conclusive on so many points. Professor Shaw, in his eloquent and somewhat inaccurate (both from the points of view of history and logic) attack on mathematical logic, urges what are, at bottom, the very same irrelevant arguments. I shall try to point out some of these inaccuracies, both because they are fairly common even now among mathematicians, and because it is surely the duty of every one to contribute as far as he can to the clarification of notions in America above all other countries; for it is from America that we expect an exceedingly large proportion of the work of the intellect in future now that Europe has deliberately handicapped herself.

Professor Shaw's slighting remark on the impotence and boasting power of logistic (p. 411) is the result of a strange misconception. Logistic deals with logical entities and deductions which are fundamental to mathematics, and it is unjust to try to make people believe that logistic ever claimed to be the overlord of mathematics. There seems, in fact, to be a note almost of personal dislike for logistic in those mathematicians who attack it. And yet the question is wholly concerned with logical facts, and is not to be answered by rhetorical appeals to prejudice or sentiment. If logic is more fundamental than mathematics, why should there be any objection to the—successful as it happens—attempt to define mathematical entities in terms of logical ones? If mathematics is more fundamental than logic, the first thing to do is to draw up a scheme showing that logical entities can be deduced from specifically mathematical ones. Until this is done, and certain objections to it are at once obvious, it is quite unconvincing to disparage cultivators of logistic. After all, logisticians are working at mathematics in much the same way that other mathematicians are. They are concerned with more fundamental problems and problems which do not so easily appeal to the public, as, say, a proof of Fermat's great theorem would, but they discover truths just as much as any other mathematicians. They introduce conceptions to work with. We may mention the idea of *propositional function* actually mentioned by Professor Shaw in terms of commendation (p. 411), which was introduced implicitly by Boole and MacColl—both early mathematical logicians—and explicitly by Frege, Peano and Russell—all logisticians. A small acquaintance with such a work as that of Frege will give plenty of examples of other powerful new ideas introduced. And then as to truths discovered by logisticians, we may remind Professor Shaw that the solution of "the paradoxes of logic" is wholly due to them, while mathematicians who were unacquainted with logistic hopelessly floundered in the search for a solution. Twelve years ago I was one of these flounderers myself, and my "solution" had been accepted as satisfactory by many mathematicians.

The real fact is that these results of logistic do not strike some mathematicians as nearly so important as some of the results of the theory of functions, for instance. I think they forget that it is only in virtue of all truths being really of equal "nobility" that Jacobi was right in claiming that a theorem in the theory of numbers was just as fine as a very striking result in mathematical astronomy.

Perhaps the greatest mistake made by Professor Shaw is the extraordinary statement about the nature of truth near the top of page 409. It is surely quite evident that truths themselves do not develop. That twice two are four was just as true last year as it will be next year,—even if no people at all are left alive on the earth next year. Professor Shaw finds fault with something I wrote because he thinks that I maintained that ideas are not created by man. It is quite evident from what I said in the context that I only held that *truths* are not created, though I certainly said in a slipshod and inaccurate way that "we do not really create *anything* in science." Really Professor Shaw shows afterward that he agrees with me that truth itself is not created, and his remark that doubtless I thought that words and ideas waited in the mines of thought for the lucky prospector does not appear to be either logical or a good guess (see pp. 409-411). However, at the top of page 409 he remarks that the world of universals changes in time. I suppose that he means that our ideas, say of an "integral" or "continuity" have changed; but I hardly think that he ought to have fallen into the error of mistaking the thing itself for a result of our groping after the thing. I take it also that he does not intend to say that truth evolves, for that rests on a confusion between a *proposition* and a *propositional function*, such as in thinking that such a function as "Dr. Wilson is President of the United States" is a proposition and not a function of the time which becomes a proposition when any instant is specified and is then constantly true or false eternally. What is the case seems to me to be that in logic and mathematics the world we are concerned with is a world of facts, not of conceptions. *Conceptions* are formed by us for the purpose of stating truths, and in the world of pure mathematics we only come across facts and *form* and *variables*. In this I think that I shall have the support of one at least among philosophers: I refer to Dr. Carus, who has always maintained that mathematics is essentially concerned with the ideas of *form* and *"anyness."*

We now come to the last inaccuracy in Professor Shaw's paper that I shall deal with. This is the question about the logic of infinity. The inaccuracy of the statements on page 412 appears clearly if we give a short statement of the facts in the treatment of infinity by mathematicians and logicians. Georg Cantor, in a series of works dating from 1871 to 1897, succeeded in founding a new and immensely important theory of transfinite numbers. The use of the lowest transfinite cardinal numbers did not and does not present any difficulty whatever to mathematicians or even logicians; but, as Burali-Forti, Russell, and others noticed in various forms the *whole* series of transfinite numbers presents difficulties which were later found to be fundamental logical difficulties of the same nature as that of the Cretan who said that all Cretans were liars. Such problems were discussed at length in Russell's *Principles of Mathematics* of 1903 and in the years after the publication of this book were satisfactorily solved by him and Whitehead. These solutions may be found in the *Principia* of 1910, and in the almost wholly symbolical form of the book last mentioned it is naturally impossible, even if it were not superfluous, that the claims made in the earlier work should be repeated. Thus it is unjust to conclude (p. 404) that the *Principia* is an abandonment of the claims of the *Principles*, brought about because of the difficulties found in Cantor's work. One might just as well conclude that the difficulties of a solution of the great difficulty of "Cantorism" had made Russell give up joking, for there are many jokes in the *Principles* and only one in the *Principia*. There is one more point. It is only what we may call a "boundary problem" about Cantor's numbers that gives rise to difficulty: the resolve that any object about which we talk or reason must be defined in a finite number of words (p. 413) does not succeed in putting out of court all classes that have an infinite number of members. Infinite classes of objects each of which can be finitely defined can be defined in a finite number of words, or better symbols of a "real characteristic." The class of prime numbers is such a class. If indeed we may use the notion of *any* (which is represented by one word) or the notion of a *variable* in general, we cannot avoid admitting definitions of infinite classes by a definite number of words. If also we may use a sign for a variable, there is no earthly difficulty in giving a general rule for correspondence in a way that is denied by Professor Shaw on page 413. The rule, for example, if n is an integer, given in the formula $n+p$, where p is another integer, indicates precisely another class of integers which is correlated to the whole class of integers considered first.

There is a small logical error committed by Professor Shaw, at least if he considers philosophy to be the same thing as metaphysics, which may explain why he is so satisfied with himself for ignoring philosophy. On page 409 he characterizes a certain assumption as "philosophical" and explicitly divides "philosophy" from "mathematics." On page 414 he agrees with Lord Kelvin that "mathematics is the only true metaphysics." Thus he would seem to hold that there is no such thing as philosophy at all; this would certainly explain why philosophical assumptions are so little worth serious discussion. Such discussion would in fact be as foolish a problem as to investigate the birthplace of Jack the Giant Killer's hen. But if we try to take a somewhat broader view, and are not satisfied with dividing our knowledge into arbitrary watertight compartments labeled "Philosophy," "Mathematics," and so on, we see that there are certain logical questions which can be and have been solved by symbolical methods which strongly remind us of algebra, which are absolutely fundamental in mathematics, and which when formulated in ordinary language sound so like what professional philosophers have often talked about that many are tempted to hurry them out of sight into the "philosophical" compartment. These are some of the questions with which logistic deals. Logistic never claimed to be able to run without the guidance of a human intellect (see p. 411) any more than the sciences of mathematics or logic or chemistry did. What it does claim to do is, like ordinary mathematics, to save our minds the labor of performing again each elementary reasoning which requires no talent but only memory—often a prodigious memory when the reasoning is complicated; so that we can reserve all the talents we may possess for overcoming those obstacles to a discovery of truth that have not been hitherto overcome. Then again, unlike ordinary mathematics, logistic seeks to point out differences in analogous ideas and reasonings which play an even greater part then analogies when we come to consider really subtle reasoning. Thus the analogy between implication between propositions, inclusion between classes, and inclusion between relations breaks down in certain cases, and we see that Russell in his later work forsakes the identical form of the symbols expressing these relations. Peano, as we know, kept to the same symbol on account of the very close analogy between the relations spoken of.

If we are content to accept without examination the arbitrary classification of people who were unacquainted with modern logic into exclusive "mathematical" and "philosophical" compartments, we must be prepared to think we see what we think are the rigid foundations of mathematics being eaten into by philosophy, and if we wish still to maintain that the foundations of mathematics are rigid we shall have continually to give the new name of "philosophy" to parts of what were hitherto considered to be mathematical. This state of things was actually brought about by what Poincaré called "Cantorism": truths which were hitherto considered solid and mathematical seemed to be thrown into doubt by the advance of philosophy. Of course this was not really so: the logical questions at the foundation of mathematics are capable of scientific investigation just as much as the theory of numbers of the differential calculus, and it is unnecessary and ridiculous to narrow the scope of our investigations because we shall meet logical difficulties if we do not. What would be thought of a tradesman who thought he could calm the mind of his assistants by maintaining that the ravages of a bull, although they seemed to be in his own china shop, were really in a drapery department which had somehow extended into that part of his shop where plates were sold? This is what those mathematicians do who dismiss awkwardness to "philosophy" and think that thereby they have kept mathematics pure and free from all "metaphysical" discussions.

Miss Dorothy Wrinch has sent the following comments on Professor Shaw's article in *The Monist* for July, 1916:

"The chief thing that I quarrel with in Professor Shaw's article is his idea of *one*: selecting one pencil from a pile is really rather different from considering the class whose members are the classes 'living kings of England,' 'fathers of A,' etc. Further, it would be difficult to give a definition of *one* or *two* which is not a statement in which one or two appears: he does not attempt to say that the other constituents of this 'statement' have not been defined, or that the definiendum is not unique. These could be his only grounds for attacking a definition, which is merely a statement in symbolic form of cases in which the number one or the number two appears. Also it seems a pity (line 9, p. 407) that he should fall into the error that he deplores in mathematical logicians, viz., the error of introducing the notion of truth (and truth value) when 'in no place.... they are defined.'

"I suppose that it was 'in the intoxication of the moment' that Professor Shaw called a propositional function of two variables a relation (p. 404, line 14), and let out of the bag the existence of a difference—hitherto, apparently, kept dark by mathematicians—between the properties of the roots of a quadratic equation and the properties of quadratic functions of x.

"Professor Shaw makes some strange remarks on page 413. If the collection of all integers does not exist (line 8) it seems hardly necessary to refute the proposition that it is possible to correlate the collection of all integers to some other infinite collections.

"It seems rather unsportsmanlike to rely upon people's short memories and call Poincaré's attacks on logistic successful. Might it not be well to remind people of the conclusions to which M. Couturat came at the end of his article in *The Monist* for October, 1912: 'Admitting the principles and primitive ideas of the logisticians, M. Poincaré has maintained that, setting out from these data, they cannot build up mathematics without another postulate—an appeal to intuition or a synthetic *a priori* judgment; and he has thought that he has discovered in their *logical* construction certain paralogisms (beggings of the question or vicious circles). I believe that I can conclude from the above discussion that not one of these theses is proved, and that, in particular, the logisticians have not committed any of the logical errors that are so lightly imputed to them.'"

PHILIP E. B. JOURDAIN.

FLEET, HANTS, ENGLAND.

THE CAL-DIF-FLUK SAGA.

EDITORIAL INTRODUCTION.

Mr. J. M. Child has given, in the following "Saga," an amusing description of the results he has arrived at in his book on Barrow, just published in the series of "Open Court Classics." The closing lines represent the opinion he has formed from a consideration of the manuscripts of Leibniz, an annotated translation of which has been appearing in current numbers of *The Monist*, beginning with October, 1916, and continued in the April number and the present one.

The saga evidently refers to the question of the invention of the infinitesimal calculus. Isa-Roba is Barrow, Isa-Tonu is Newton, Zin-Bli is Leibniz, while Cavalieri is mentioned under the name of Ler-a-Cav. Gen-Tan-Agg stands for Barrow's *Gen*-eral method of *Tan* gents and of *Agg*-regates; while Shun-Fluk and Cal-Dif obviously refer to the methods of Newton and Leibniz. Batnac is the ordinary abbreviation of the Latin for Cambridge, Cantab., with its letters reversed; and the allusion in the next line is to Barnwell Pool, where it is stated that an undergraduate whose boat had overturned was saved from drowning, but died soon afterward from blood-poisoning! Terangel is a transformation of Angleterre, i. e., England. Ris-Pah is Paris, where Leibniz lived at the time of the invention of the calculus.

In the second stanza, the allusions to "burning midnight oil," the quill pen, incandescent gas mantle, and the electric light are all fairly obvious; while the Swan may be taken to refer to a well-known make of fountain pen. Stanza 5 refers to the publication of a book. The archery in the first method of training alludes to the ancient definitions of a tangent and a normal to a curve; and the sword-play recalls *Euc.* I, 10 and *Euc.* I, 1, while the allusions in the second are easily referred to the method of indivisibles.

In Stanza 9, the dagger refers to the differential triangle, which Barrow only included in the first edition of his work on the advice of Newton; the knobs on the hand-grip refer to Newton's "dot" notation.

The two weapons of Zin-Bli are the signs invented by him for differentiation and integration. Lastly, Li-Nu-Ber is John Bernoulli, who stated that Leibniz got the whole of his fundamental ideas from Barrow, whereas Leibniz himself denied any indebtedness to Barrow.

THE CAL-DIF-FLUK SAGA.[1]

1. Saga of sons of a Goddess, of Thought and Learning the fountain,
(Haply in that which I sing, a real historical meaning,
Wrapped in a fanciful garb, and oddly disguised as a saga,
Those who are skilled in lore, and erudite more than their fellows,
Knowing the facts of the case, if they diligently seek may discover.)
Dwelt She, She dwells upon Earth, and henceforth for ever and ever
Dwell so She will among mortals. 'Tis thus decreed by the All-Wise.

2. Oil from the Midnight Lamp the sacrifice burned on her altars,
Plumes from the wing of the Goose her now peculiar token;
Not so at first was it thus, and not in the times that are coming

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[1] From a manuscript found in 1916 A. D., while searching an ancient tumulus or "barrow," and made out from the original by J. M. Child.

Will it be Oil and Plume; I see with the eye of the seer
Wondrous visions of Light, enwrapped in a Mantle resplendent,
Torn from the heart of a stone, the essential soul of the Sun-god
Prisoned for ages therein; and globes of crystal translucent
Glowing with filaments bright kept hot by the Spirit of Lightning,
Swan of the Golden Beak instead of the Goose for her token.

3. Sent upon Earth to dwell with mortals by will of the All-Wise,
Children divine to bear to those who Her fancy might capture.
Ardent and long was the wooing, both strong and patient the lover,
Ere he received his reward, or ere She presented him offspring.
Else as a mark of Her love to him She had chosen to honor,—
Chosen for womanly whim, for some unaccountable reason
Honored above all else, who never had courted her favor—
Sent She on lighting wings the soul of Her heart, Inspiration.

4. Children of fathers of Earth, but endowed with the life of the Mother,
Destined as Heroes to wage perpetual warfare on all things
Troubling the minds of men desiring to widen the limits
Set on the realm of We-Know, by the race of the children gigantic,
Issue of Never-Before out of We-Never-Heard-of the-Method.
Children begotten from Her are known by the names of their fathers,
More by the deeds of the sons are the fathers so held up to honor;
Accurate records are kept; thus long through the ages that follow,
Known by the deeds of the sons are the fathers so held in remembrance.
Rightly was this the Law, for responsible he for the training,
Fitting the son for the fight for freedom and fuller perception.

5. Till 'twas such time as was meet, the custom obtained that in secret
(Jealous that others might see not fully developed the power
Promising greatness to come), this fatherly training continued
Day after day for an eon; until with a flourish of trumpets,
Front of the eyes of all, tattooed with the symbols of Learning,
Clad in a mantle of calf-skin, bearing on back and on bosom
Plainly for all to observe, in resplendent gold letters, his title,
Son of the Goddess of Thought, was he set as a champion of Knowledge.

6. The methods of training were two, at least only two were accounted.
Oldest and best known of all was the method derived from the Ancients,
Cumbrous, exhaustive and long; horizontal and parallel bar work,
Drawing of cord of the bow, and the rings were considered essential;
Accurate hand and eye were developed by shooting an arrow,
Grazing the cheek of a figure, or forth from it standing erected;
Cleaving a bar into twain, so each part as to balance the other
(Nought but two measuring swings ere the cut was delivered allowed him).
Such like in days of old had fitted the Heroes for battle.
Founded on this was the second, but strangely unlike it in practice;
Suppleness rather than strength was the object and creed of the trainer.
Straight-edged still was the sword; with it blocks were sliced into shavings,
Shavings were sliced into threads, and threads were chopped into pieces,
Parts of ineffable smallness, divisible reckoned no further.
Masonry part of the course, in which arches with bricks were fashioned,
Leaving the corners undressed; as the pupil advanced in his training,
Smaller and smaller the bricks, indivisible finally counted.
Specially fitted for Heroes, prepared for attack on the giant
Clans of A-Re-A and Vol-Yum, the brood of Cur-Va-Rum and Mez-zur.
Failed if the fatherly training, the Goddess in sorrowful anger
Took from the child his soul, the gift which at birth She had given,
Worthier father to bless, if ever another such won Her.

7. Once in the days now gone, there lived on the banks of the Batnac,
Renowned for its smells and its mud, where pollution enters at Well-Barn
(Truly not then was this fame, nor yet at the time of this writing
Thus had it won a repute, 'tis a prophecy sure that I utter),
Land of Terangel within, a mortal yclept Isa-Roba.
Many and varied his loves, his fickleness surely a drawback;
Truly a wonder it was that the Goddess e'er let him approach Her.
Bare She however a son, Isa-Roba undoubted the father,
Fair both in face and in form, a divine conception befitting;

Ne'er such a babe before was born with so splendid a future;
Seemed that the soul of his Mother had enter'd the Child at his birth-time:
Best that She had to give, best that She can give for all time,
Gave She this son of Her heart; Gen-Tan-Agg Isa-Roba did name it.

8. Trained he the boy in a manner that savored of that of the Ancients,
Discipline rigorous keeping, yet toned with a method that fore-time
Ler-A-Cav brought to perfection, a mingling of first and of second
Systems of training recounted; however 'twas foredoomed to failure.

9. Hercules never so strong as the youth Gen-Tan-Agg, no, nor Samson.
Armed with his two-handed weapon he met many giants in combat;
Numerous clans he defeated, by slaying their general doughty.
Nevertheless were his muscles too stiffened by reason of rigor,
Due to the manner in which Isa-Roba conducted his training.
Love for the two-handed broadsword, with which Isa-Roba had armed him,
Made him neglect the superior weapon that hung at his waist-belt,
Sharper by far than the sword-blade, a steel of superior temper;
Seems Gen-Tan-Agg only used it preparing the shafts of his arrows;
Nigh came to leave it at home as he set out upon his first journey,
Girding it on at the last, not perceiving in it that a weapon
Ready to hand he had got against which no armor of mortals
Could for a moment prevail; for piercing the joints of the harness,
Off'ring no passage to sword-blade, it reached his opponent's main vitals;
Forced him to give up his treasure, the secret protected for ages.

10. Happened it thus that a Hero, high-blessed by the Goddess his Mother,
Spoiled by the weapon mistaken his anxious sire recommended,
Fame and renown and great honor did miss for ever and all time,
Losing the chance that was offered, a name and a high reputation.
Lastly, by father discarded (who fickly returned to a first love),
Languished and nigh came to perish, unhonored, unsung and neglected.

11. Some of the records of giants the youth Gen-Tan-Agg had defeated

Chanced Isa-Roba, however, had told to a friend Isa-Tonu;
Agile by nature, the latter immediate saw that the dagger,
Superior far to the broadsword, was a weapon of magical value;
('Twas Isa-Tonu's advising that just at the very last moment
Caused Isa-Roba to add to his offspring's armor the dagger).
Pity, perhaps, for the youth, or a covetous eye for the poignard,
Caused Isa-Tonu to take neath his fostering care the young stripling,
Freeing his father from trouble, unhampered to follow his fancy.
Thus Isa-Roba the story departs from, unhonored for all time;
Save and if only in future, this tomb may be opened by some one
Trying to find out the truth of the Hero's father and birthtime.
Under the fostering care of a trainer less hide-bound by nature,
Slowly at first, then apace, did the Hero recover his power.
Changed was his armor, the sword altogether replaced by the dagger,
Changed was the dagger in form, for a knob, sometimes two, on the hand-grip
Gave it a far better balance. Obsessed by his special requirements,
Secretly long Isa-Tonu did bind Gen-Tan-Agg to his service.
Later ungratefully hiding the name of the Hero who served him,
Swearing that all had been done by his own bastard offspring, young Shun-Fluk.

12. Thus once again was the Hero discarded and left for to languish,
Shun-Fluk attaining the fame that should his have been truly and rightly.
Nemesis, son of old Equity, sternest of Gods and the justest,
Saw Isa-Tonu's deception, and straightway the Goddess of Learning
Sought He and told Her the story. In sorrowful anger the Goddess
Listened with eyes that flamed at the failure that followed Her offspring,
Due to his father's bad training, and then Isa-Tonu's enslavement;
Listened and cursed the first, for the other a punishment thought out.

13. "Punishment dreadful and dire!" So she spake, the while Nemesis listened,
Listened and nodded and smiled, as approved He the plan She suggested.
"Lives there a mortal in Ris-Pah, who long has courted my favor;
Often of late have I thought that at last I'd reward his devotion.
Lacks he but one little thing, only one thing to render him fitting

Trainer of offspring of mine; but the lack mean I now to forgive him.
Never again could I bear such a child as I bore Isa-Roba;
Certain is that; but immortal the soul that at birth-time I gave him,
Breath of my life, Inspiration, again, Gen-Tan-Agg expiring,
Can, if I will it, enlighten the child which I'll offer to Zin-Bli.
Thus is he called by mortals, an inventor of weapons and symbols.
One has he fashioned already, in shape like a chopper for fire-wood,
Straight in the shaft, with a hand-stop to stay it from slipping,
Circular edge to the axe-blade, to shaft is it fastened by bolt-head;
Much like the symbol that mortals set fourth in the lower-case system.
This shall he teach my offspring to use to more delicate purpose.
Binds he his sticks all together with cord made out of the sum-omn;
Lurking however in thought is the germ of a better invention,
Rod with curl at each end, slightly bent, so that clipped round the bundle,
Binding the whole into one, he is able to thus grasp it firmly.
Armed with each of these twain, shall his offspring forth stand as a Hero."
Spake She, and Nemesis nodding to all His approval, it was so.

14. Cal-Dif named Zin-Bli the child, and he trained him these weapons to master;
Speed, at all rates, with the first he created new records completely,
Nor did he stay at that; with the second, the brood of the giants,
Laid he them low in the dust, so that never again should they trouble.
All that the Goddess had said, so performed She; the credit of Cal-Dif
Famed through the kingdoms of mortals, became a renown for the father,
Ne'er to be equalled till Earth is devoid of reasoning mankind.

15. Swelled as to head by renown, though Zin-Bli well knew Inspiration
(Could he forget this?) had wrought in a magical manner the marvel,
Yet could not bear it for others to know whence the source of his wisdom;
Denied he the source whence it came, Isa-Roba's offspring discarded.
Nemesis saw what he did, and he stirred up the folk of Terangel,

Shun-Fluk to accuse him of stealing and sending him forth as his
Dif-Cal.
None seemed to have guessed the truth, save a man by the name of
Li-Nu-Ber.

16. Ye who perchance may consider this saga in future far ages,
Know now the truth ye may; that the soul of the Goddess of Learning
Entered at first Gen-Tan-Agg, but he languished for lack of good
training;
Afterwards, renamed Shun-Fluk, he recovered some of his birthright;
Dying, his soul was then given to an ordinary child of a mortal,
Rendering its face and its form like one of divine conception.

17. Accepted as such by all, till the day that this saga's discovered,
Haply e'en then, for foretell I that Cal-Dif................

Unfortunately, the manuscript, which consists of another couple of sheets that were outermost in the roll, here becomes indecipherable through being destroyed by damp; it would have been interesting, and useful in the light of judging of the truth of the facts given, to have verified how far the prophecies were fulfilled by events since the time at which they were written down and the manuscript hidden in this old burial-mound.

J. M. Child.

Derby, England.

NOTES ON DE MORGAN'S BUDGET OF PARADOXES.

In a work requiring the large amount of reading involved in editing a book like the *Budget of Paradoxes,* and particularly in the condensing of the results to the proper proportions for footnotes to aid the reader, it was, of course, inevitable that a certain number of inaccuracies would occur. It is also evident that many more notes might profitably have been added to elucidate the meaning of the text, or to correct the original where this would be warranted.

De Morgan was a careless writer and many of his errors are mentioned in the footnotes; but numerous others exist, some of which are patent to any reader and others of which might profitably have been set forth by the editor. It is also a serious question as to whether the translation of common phrases is not more of a hindrance than a help to even the casual reader, and whether the space used by such translation might not have been more profitably devoted to a further elucidation of the text. This is the feeling of one or two critics.

Since the work was published, several friends have called attention to a few misprints, a few generous critics have suggested helpful changes, and one or two others have objected to certain of the notes. It therefore seems proper to present a few emendata and errata which may assist the reader of the work.

In the matter of emendata to De Morgan's text itself and of suggestions as to further helpful notes I am indebted chiefly to Prof. A. E. Taylor of St. Andrews, Scotland, who has gone over the work with great care and has kindly given the Open Court Publishing Company the benefit of his reading. The following notes on De Morgan's text are due to him.

Vol. I, page 3. De Morgan should not have attributed to Spinoza the anonymous *Philosophia sanctae scripturae interpres.* It was probably the work of his friend and physician Lodowick Meyer.

Vol. I, page 41. De Morgan's version of the passage from the commentary of Eutocius on the tract by Archimedes on the measure of the circle is not satisfactory. The Cerii of Porus should be the Ceria (*κηρία*, honey combs) of Sporus. He probably used the Wallis edition of Eutocius and quoted only the first four words of the passage (*Archimedis Opera Omnia,* III, p. 300, of the 1881 edition of Heiberg): *εἰς ἀκριβεστέρους ἀριθμοὺς ἀγαγεῖν τῶν ὑπ' Ἀρχιμήδους εἰρημένων, τοῦ τε ζ″ φημὶ καὶ τῶν ι′ οα″*. The restoration adopted by Heiberg makes the statement of Eutocius correct: "a more accurate evaluation than that of Archimedes, i. e., than the fractions $\frac{1}{7}$ and $\frac{10}{71}$." According to Sporus, Philo of Gadara had found closer limits. Archimedes had given $3\frac{1}{7}$ as the upper limit and $3\frac{10}{71}$ as the lower limit of π, the ζ″ and οα″ representing merely the fractional parts.

Vol. I, page 96. De Morgan's language seems to imply that the Convocation of the University of Oxford is, or was, a body of ecclesiastics of the Anglican Church, but it is not an ecclesiastical body at all. It consists of all masters of arts who qualify by the regular payment of their university dues. Professor Taylor suspects that De Morgan may have confused the Convocation of Oxford with the Convocation of the Clergy of the Province of Canterbury.

Vol. II, page 274. For De Morgan's translation of *οὖλον μέλος*, read "a song of bale" (*ὀλοὸν μέλος*).

Vol. II, page 277. De Morgan overlooks the true reason why Pope scans *Mathesis* as *Máthesis,* namely, that like all writers of his day he pronounced Greek names according to their accent, not as we now do with an adjustment of the stress accent to the quantity of the vowels.

Vol. II, page 322. De Morgan is incorrect in his statement as to Böhme's division of Mercurius. Böhme divides it Mer-cu-ri-us, not Merc-u-ri-us.

Vol. II, page 340. It would be interesting to know whether De Morgan's complaint that Walter Scott did not know what "Napier's bones" were is well founded.

Professor Taylor suggests various other interesting notes relating to the text, and of course such a list could easily be extended.

In the extensive bibliography given in the notes it was inevitable that certain slips of the pen should have occurred. In Vol. I, page 105, I followed Bierens de Haan in giving the spelling "Johannem Pellum." My friend Herr Eneström has a copy of the edition in question and the spelling there given is "Ioannem Pellivm." He also calls my attention to the proof given in the *Bibliotheca Mathematica* recently that Mydorge was not the author of the *Récréations mathématiques* as published in Boncompagni's *Bullettino.*

Among the slips of the pen which I have noticed since the work appeared is the name of D'Alembert for that of De Lalande in Vol. I, page 41; "condemned" for "contemned" on page 92; and, in Vol. II, "blata" for "beata" on page 61.

Professor Taylor calls attention to the further slips of "fellow of Cambridge" for "fellow of Trinity College, Cambridge" and of "Derion" for "Denon" (Vol. I, page 76); "Viscount of Palmerston" for "Viscount Palmerston" (page 290); "closed" for "classed" (in the text, Vol. II, page 148); "tolo" for "toto" in the text (page 344); and ± for ± 1 in the text (page 368).

I am also indebted to Professor Taylor for several suggestions of betterment of the translations, matters which should have been attended to by me in the preparation of these particular notes even though I entrusted this work to another. The following changes are not to be attributed to him, although changes (sometimes more extended) were suggested by him.

In Vol. I, page 3, for "what it was" read "that it was"; page 40, for "its appointed path" read "the appointed path"; for the free translation in verse on pages 53-54, for "And lacking nothing but a start, and lacking nothing but an end," read "The only one without a start, the only one without an end"; page 339, for "think himself to die" read "feel himself dying."

In Vol. II, page 23, n. 4, for "He was wont to indulge in" read "He has a habit of refreshing his reader by"; page 151, for "condemned soul" (literal) read "hack" (colloquial); page 154, change the translation of the familiar legal phrase to bring out the pun upon J. S., "Summum J. S. (for *jus*) *summa injuria*" (the height of law—J. S.—the height of wrong); page 200, change "sleeping power" to "sleep-producing power"; page 228, translate διος εἰμι ἢ ἥρας, as "of Zeus I am, or Hera," and ἢ μίσσα as "mass"; page 260, translate the quotation from Acts xix. 38, as "the courts are sitting"; page 262, for "according to which" read "relatively"; page 283, for the manifest error in the note on "*ab ovo*" read "from the egg," probably relating to the passage in Horace, "*nec gemino bellum Trojanum orditur ab ovo*," or possibly to "*ab ovo usque ad mala*"; page 365 for "slayst" (misprint for "slayest") read "keepst."

Professor Taylor also suggests that Hobbes lived only about eleven years in France (Vol. I, page 105); that Burnet left England to avoid being involved in the ruin of the Whigs (page 107); that Street acted in accord with the law (page 124); and that there was nothing strange in Laud's patronage of Palmer (page 145). The details of these emendata and certain other suggestions of change would trespass too much upon the space which the editor of *The Monist* has kindly allowed me.

DAVID EUGENE SMITH.

TEACHERS COLLEGE, NEW YORK.

BOOK REVIEWS AND NOTES.

REFLECTIONS ON VIOLENCE. By *Georges Sorel.* Translated, with an introduction and bibliography, by *T. E. Hulme.* London, George Allen & Unwin, 1916. 7s. 6d. net.

Sorel's book is exceedingly difficult to discuss in a short review. Its substance is a very acute and disillusioned commentary upon nineteenth-century socialism, and upon the politics of the French democracy for the last twenty-five years. It contains also two elements which must not be confused, Sorel's own political propaganda (if he would allow it to be so called) and his philosophy of history formed under the influence of Renan and Bergson. And it expresses that violent and bitter reaction against romanticism which is one of the most interesting phenomena of our time. As an historical document, Sorel's *Reflections* gives, more than any other book that I am acquainted with, an insight into what Henri Gheon calls "our directions."

Doubtless many readers will be disposed to consider the book under its first aspect only. But the study of Sorel's political observations requires an accurate knowledge of government and parliamentary activities since the Dreyfus trial, and does not in itself make the work of importance to the English and American public. What Sorel wants is not a political, but a *social* form. One must remember that his creed does not spring from the sight of wrongs to be redressed, abuses to be cured, liberties to be seized. He hates the middle classes, he hates middle-class democracy and middle-class socialism; but he does not hates these things as a champion of the rights of the people, he hates them as a middle-class intellectual hates. And the proletarian general strike is merely the instrument with which he hopes to destroy these abominations, not a weapon by which the lower classes are to obtain political or economic advantages. His motive forces are ideas and feelings which never occur to the mind of the proletariat, but which are highly characteristic of the present-day intellectual. At the back of his mind is a scepticism which springs from Renan, but which is much more terrible than Renan's. For with Renan and Sainte-Beuve scepticism was still a satisfying point of view, almost an esthetic pose. And for many of the artists of the eighties and nineties the pessimism of decadence fulfilled their craving for an attitude. But the scepticism of the present, the scepticism of Sorel, is a torturing vacuity which has developed the craving for belief.

And thus Sorel, disgusted with modern civilization, hopes "that a new culture might spring from the struggle of the revolutionary trades unions against the employers and the state." He sees that new political disturbances will not evoke this culture. He is representative of the present generation, sick with its own knowledge of history, with the dissolving outlines of liberal thought, with humanitarianism. He longs for a narrow, intolerant, *creative* society with sharp divisions. He longs for the pessimistic, classical view. And this longing is healthy. But to realize his desire he must betake himself to very devious ways. His Bergsonian "myth" (the proletarian strike) is not a Utopia but "an expression of a determination to act." The historian knows that man is not rational, that "lofty moral convictions" do not depend upon reasoning but upon a "state of war in which men voluntarily participate and which finds expression in well-defined myths." It is not surprising that Sorel has become a Royalist.

Mr. Hulme is also a contemporary. The footnotes to his introduction should be read.

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THE NEW INFINITE AND THE OLD THEOLOGY. By *Cassius J. Keyser.* Yale University Press, New Haven, 1915. Price 75 cents.

In this essay Dr. Keyser shows many interesting ways in which some of the most difficult problems of theology may be partly or wholly overcome by mathematical means.

The relation between religion and science is discussed, the author showing that while science belongs to the middle zone, or rational world, religion belongs to the over-world or superrational. Then follows a brief discussion of the relation of theology to religion, theology being primarily a science, in a word "the science of idealization." From the purely theological standpoint, "God is an hypothesis." In all definitions of God the notion of infinity is foremost. Therefore the essay develops the mathematical concept of infinitudes and through many examples makes clear the denumerable type of infinite manifolds; then far surpassing this in glory, the continuum type, and points to types of even higher orders. "The infinite of theology is the limit of the endless sequence of more and more embracing infinitudes presented by science."

The contradictions of theology are of two kinds, foreign and domestic. Theology may rid herself of the foreign variety by casting out all illegitimate postulates. In the world of infinitudes the part of a group may be just as numerous as the whole group. So in the realm of theology, the seemingly contradictory ideas of omniscience and freedom may be reconciled; for the dignity of omniscience is as great as omniscience itself. The same line of reasoning is applied to the doctrine of the Trinity. The essay closes with a reference to the so-called domestic difficulties, and shows that a being may have many contradictory aspects and yet viewed in a large way all these aspects may be true; just as in comparing different systems of geometry built on various foundations the mathematician finds contradictory facts, yet does not doubt the truth of any of these facts.

Dr. Keyser's careful, earnest style of writing makes it a pleasure to read his works, and any one who has the "mathematical spirit which is simply the spirit of logical rectitude" will enjoy this unusual essay.

EMMA K. WHITON.

THE STUDY OF RELIGIONS. By *Stanley A. Cook, M.A.*, Ex-Fellow and Lecturer in the Comparative Study of Religions and in Hebrew and Syriac Gonville and Caius College, Cambridge. London. A. and C. Black. 1914. Price, 7s. 6d. net.

Mr. Cook is very long-winded, but in spite of dryness and abstractness of style he has written a valuable book. Much thought has evidently gone into it, and its defects are due to a difficult manner of exposition, not to poverty of ideas. This is not an "Introduction" of the type of Jevons's book: it gives no data for the beginner, nor, as one is apt to expect from the title, does it deal chiefly with primitive religion. It is rather the comments of a scholar—Mr. Cook is a recognized authority in his field—on the aims and methods of his study. He has a great deal to say, and much that is extremely good, on the evolution of religion—as is indicated by several chapter headings: Survivals, The Environment and Change, Development and Continuity. "The doctrine of survivals," Mr. Cook says, "is entirely inadequate when it forgets that we are human beings and do not accept beliefs merely because they happen to lie within our reach. The doctrine of survivals, is, in fact, a very handy and cheap explanation of some one else's beliefs and practices—hardly of our own!" Survivals are not simply "left behind," they are subconsciously selected. Mr. Cook warns very wisely against arguing from the part to the whole, against constructing a hypothetical system into which every survival must fit. He warns also against confusing the evolution of beliefs with the evolution of environments, in judging apparent retrogressions. On the critical attitude, on the acceptance of data, Mr. Cook has some excellent observations, and on the historical versus the religious importance of critical revisions. He holds that the present is a time of religious unrest, though like most of us, he cannot point to any definite theology for the future. His conclusion is as follows: "The unbiased student of religions can hardly escape the conviction that the Supreme Power, whom we call God, while enabling man to work out, within limits, his own career, desires the furtherance of those aims and ideals which are for the advance of mankind." ρ

Just as we are going to press we receive two additional notes from Dr. W. B. Smith to be inserted in his article as indicated respectively on pages 330 and 337.

Page 330: "For which Rutherford's 'nucleus theory,' apparently required by the facts in the scattering of 'alpha rays' (of helium atoms) in passing through laminae, substitutes a positive electric core, extremely minute, for gold only one trillionth of an inch in diameter, in volume one billionth of the atom itself. It would seem that the negative electron is nearly six thousand million times as large as the positive hydrogen core. For Thomson's later views see *Philos. Mag.*, 1913, p. 892."

Page 337: "Why do the members fall together to the center as their energies are dissipated in electric radiation? Bohr (*Philos. Mag.*, 1913, pp. 1, 476, 854) invokes Planck's 'Quantum'-hypothesis in solving this riddle."

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THE MONIST

WHAT IS A DOGMA?*

EDITORIAL INTRODUCTION.

The primary significance of a dogma is not its speculative content, but the speculative truth of dogma is expressed in terms of action. Such is the proclamation of a Roman Catholic thinker which has evoked a lively discussion, and although his work has been placed on the Index, this has evidently been for other reasons than any connected with the charge of heresy. For this thesis defines the general concept of dogma in the expressions of the well-known philosophy of action originated by Maurice Blondel and published in his book *L'action* which appeared in 1893, and as far as we know his book was not placed on the Index. "Perhaps," writes Father E. Bernard Allo, O.P., "the thesis sketched by Le Roy is not so different, perhaps the divergencies are less in idea than in expression, in the *significat* itself than in the *modus significandi*" (*Foi et système,* Paris: Bloud et Cie., 180, 181), and this is confirmed by Le Roy himself in a footnote on page 70 of his *Dogme et critique.* A. Houtin in his history of Catholic modernism mentions the Rev. A. D. Sertillanges as expressing the same opinions in the referendum on Le Roy's article on dogma as Father Allo, and so far as we can ascertain, their writings have not been placed on the Index. Further, for a book to be placed on the Index does not mean that it is condemned, but the authorities intend to say that for some reason *hic et nunc* the book is not to be generally read.

This article of M. Edouard Le Roy entitled "Qu'est-ce qu'un dogme?" has even been looked upon with favor in some quarters by representative ecclesiastical authorities; and being of great importance, not only for Roman Catholicism, but also for Protestantism, yea generally for all religion, we take pleasure in rendering it accessible to English readers.

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* Translated by Lydia G. Robinson from the sixth French edition of the author's book *Dogme et critique.*

It first appeared in the French fortnightly journal *La Quinzaine* of April 16, 1905, where it was accompanied by an editorial note as follows: "Without expressing any decision on our own part with regard to the opinions of M. Le Roy it seems to us both interesting and useful to take a text from his work by which to invite theologians to furnish the public with the elucidation he asks for. Hence we address a special invitation to all the authorized specialists in Catholic theology, to the professors of our liberal universities and of the larger seminaries, to religious orders, and to the priests."

The invitation was eagerly accepted, and seven later numbers of *La Quinzaine* contained communications of varying importance on the subject. But these formed only a small part of the discussion raised by this striking article. Its publication was followed by a vast array of controversial writings which continued with increasing violence throughout an entire year. Twenty or more other journals opened their pages to the subject; not only such distinctly clerical journals as *Etudes, Revue thomiste, Revue du clergé français, La Croix,* etc., but also general philosophical reviews, *La Pensée contemporaine, Revue de philosophie,* and such liberal journals as *La Justice sociale, Le Peuple français,* and *La Vérité française*. And not only these religious and critical periodicals devoted their pages to the subject but a well-organized opposition to the offending article rushed into print through the daily press.

Still the question which the author put to the clergy in deference to them as being officially charged with the instruction of the people did not receive a satisfactory answer. Many heaped M. Le Roy with malicious calumnies, and many honestly misunderstood him. Many too misjudged him because they knew of the article only through garbled reports or hostile criticisms. He therefore considered it necessary to put the article in permanent form, and so he published it in a book entitled *Dogme et critique* (in the series *Etudes de philosophie et de critique religieuse* with Librairie Bloud et Cie.) together with his published replies to the most important of his adversaries, a careful bibliography of the controversy and a more detailed development of the most significant points of his thesis in fourteen brief additional chapters.

* * *

Religion is a practical affair, and its main purpose is to serve us as a guide through life. Religion as a sentiment is practically universal and we may consider it to be innate. It is a panpathy or all-feeling which produces in every individual a deep-felt longing to be at one with the whole universe of which each is a part.[1] As every material particle is an embodiment of gravitation in proportion to its weight, and is possessed of a well-apportioned pressure somehow and bent some whither, so the souls of things existent feel themselves parts of the great whole in which they live and move and have their being.

This panpathy in its historical development under definite conditions assumes a definite form, and so religion leads necessarily and naturally to church life and church formation, with dogmas and regulations of conduct.

The dogmas of the church are collected in what has been called the symbolical books which accordingly contain the several confessions of faith. They are called symbolic because they served as symbols, or tokens of recognition to the members of the church. The man who could recite the symbol was welcome in the congregation as a brother who cherished the same faith, having found the same solutions of the world problem as the whole church and having accepted the same formulation of it.

The dogma is a symbol, but it is more than a symbol; it is an appropriate symbol. It is a statement satisfactory to the whole congregation and in so far as it is satisfactory to the whole congregation it has become to them a truth.

Dogmas are truths. Being religious truths they are holy truths, and since they are taken seriously, they have often become the cause of much controversy and have led to quarrels and bloodshed, to persecution and warfare, to the establishment of the inquisition and denunciation of heretics. We now learn that the intellectual feature of the dogma is derived from the main and essential feature, its practical value. This is an enormous gain, for it introduces into the nature of dogma a condemnation of all intolerance and establishes an unlimited freedom of interpretation without, however, detracting a hair's breadth from the practical significance of the dogma. Not one jot or one tittle shall pass from it, but a thinker is allowed to construct its meaning as best he can, provided he recognizes and holds on to its practical application.

God is our father; he is called upon in prayer as a personality

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[1] For a more complete definition of religion in its several phases see Carus's *Dawn of a New Era*, pp. 96-97.

—not a human personality, but a divine personality. The interpretation of personality is a problem by itself, but the significance of the dogma "God is a person" means that we should adjust our relation to God in such a way as to make it a personal relation, and this practical application constitutes the primary and underived significance of the dogma.

This view is not a loose way of treating the dogma; for the freedom of interpretation gives much liberty of speculation, but not an unlimited license. It is restricted and allows the dogma to stand and remain unalterable as the only possible, the only allowable, expression of a truth. Though the dogma is not absolute it is definite, and any other formulation of it would be wrong and must be rejected. Thus the view of dogma here represented by M. Le Roy remains as uncompromising as ever and would not allow any dillydallying for the benefit of speculative minds.

It will be sufficient to characterize the author's effort and the misunderstandings created in the broad problem in his own words. They will show first the sincerity of his undertaking and explain the situation of his own mind, and secondly they will describe his critics and their inability to grasp M. Le Roy's point of view. A faithful Catholic's understanding of the nature of dogma is characterized by the article itself and for a summary of this phase of religious thought it is fully sufficient.

This is what our author says in speaking of himself (*Dogme et critique*, pp. v-x):

"On April 16, 1905, I published in the *Quinzaine* an article entitled 'What is a Dogma?' in which, speaking as a philosopher who desires to think his religion, I addressed various questions to theologians and apologists.

"Why did I use the form of interrogation instead of a direct exposition? In deference to those who have official charge of instruction. It seemed to me desirable that the reply should come from them. In this way I hoped to manifest my intention to act always in conformity with the hierarchical principle divinely established in the church. Although I have scarcely been able to congratulate myself on the reserve and courtesy I thus showed, since some have been pleased to see in it only a caution lacking in courage and candor, still I retain to-day the same way of looking at things. But be assured this does not in the least mean that I experience the slightest difficulty for my own part in reconciling faith and reason, nor that I hesitate or doubt the least bit in the world with regard to my duty as a Catholic.

"My aim was, briefly, to expose certain *facts* which I had had the opportunity to observe around me, and also to report an *experience* I had had in my relations with the unbelieving intellectual world. It was for the theologians, I thought, to declare themselves after discussing the plan which I submitted to them. As for myself, I was only a witness testifying to what he had seen and come in touch with, a Christian soul relating some of the steps it had taken.

"This attitude has been misunderstood. It has been regarded as craftiness or malice, as a challenge or an irony. Some one spoke with reference to it of a question 'irreverently and even impertinently stated.'[2] Was not 'importunately' meant instead, without daring to say it, or admitting it? For, I beg to inquire, how may one set about being more deferential than I have been? Unless the only deference that is acceptable and sufficient is the deference of an indifferent or heedless silence. Is it true that the question asked was indiscreet? Certain papers hastened to make the claim, and the *Siècle* for instance was much diverted at the idea of Catholics not being able to agree on defining a dogma. These are certainly not my own sentiments. In asking an explanation I never intended to be, nor do I think I was, a trouble maker, disturbing slumber or ruffling tranquillity. But words like those I have mentioned tend to justify this ill-natured hypothesis, and therefore it is they which in the final analysis I find lacking in courtesy.

"For my part, on re-reading what I have written and feeling ready to write again, I declare with M. Fonsegrive:[3] 'Have we been wrong in saying these things out loud and, being Catholics, in having enough confidence in our religion, in the power of truth, to dare speak frankly, clearly, even vigorously? Would we have shown more regard for our beliefs if we had spoken timidly and feebly as one speaks at the bedside of the dying?' One must indeed stand up for oneself. We are neither dissembling Protestants nor disreputable rationalists. We are only searching always for the greatest religion, without concessions or haggling. We do not wish in the least to be either rebels or even eccentric persons. But our faith is firm enough for us not to fear to look the facts in the face and to speak out clearly what they show us; and we attach enough value to the divine word to wish to think with all the strength of our soul, assured in advance that there we will find life and light without other limitations than our own. Moreover we feel that we are enough protected by the living supremacy of the church to preserve the most complete internal peace throughout our most venturesome inquiries. We are, in fine, sure enough of our obedience to legitimate authority to have no fear in running the commendable risks which the experience of life always entails. But the obedience we intend to render is not a simple obedience of formulas and motions, it is a profound obedience which lays hold of our whole being, heart, will and intelligence—in short, an obedience of reasonable men and free agents, not of slaves or mutes.

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[2] *La vérité française,* Dec. 20, 1905.

[3] *Quinzaine,* Jan. 1, 1906, p. 30.

"Nevertheless, as soon as the article 'What is a Dogma?' appeared a vast array of controversial writings began which continued with increasing violence during one whole year. Not only did the reviews take part, as was their natural business, but the daily papers as well. For after having reproached me for opening a discussion on such a subject before a public which though educated was not professionally qualified, they had nothing more urgent to do than to force the discussion before the eyes of a crowd which this time had neither proficiency nor culture. The organization of the exposure was perfect and the matter was abundantly exploited by those who make orthodoxy a monopoly or a standard and who are always to be found upon the heels of any one who takes the liberty of thinking for himself.

"To polemics conducted in this way I shall make no reply. Their authors, in spite of the pretensions they parade, are representative of nothing in the church, and as, on the other hand, they do not discuss but condemn and anathematize, substituting injury, slander or denunciations for arguments, they are representative of nothing from the intellectual point of view. What separates us from them is a question of morality much more than a question of critique.

"Fortunately other questioners have made their voices heard, loyal and disinterested questioners of broad minds and upright hearts, striving to understand and seeking nothing but the kingdom of God, the welfare of souls, the light of truth. The present volume is dedicated to them, to them and all those, whether known or unknown, who are like them. Is there any need of justifying oneself otherwise than by the words of Fenelon, which he might have taken for a motto: 'Every Christian, far from entering controversies, ought instead to explain his position more and more to try to satisfy those who have had trouble with the first explanation.' If this motive is not sufficient I may add that I cannot remain indifferent in the face of the opinions that have been attributed to me. Too many people have become acquainted with my article only through incomplete analyses, through prejudiced reports or through refutations which may well confuse them; it is important that I should publish an authentic text with comments made necessary by the publicity the controversy has attained.

"For the rest, I still retain the same attitude I had at the beginning. I wish to put a question, nothing more. The accompanying comments and reflections are only to elucidate the meaning and the scope; to show also that it has not in the least been adequately answered; finally to furnish a definite theme for discussion and investigation. Who would dare to find occasion in this to accuse me of heresy?

"And now I have finished my task on this point; I have said what I had to say. The question has been asked, and nothing could prevent it from being asked. Henceforth the ideas will make their way of themselves and nothing will stop them. Let the future answer. Perhaps we shall soon see what has often happened before, that what was once regarded as bold and disgraceful will end by being universally accepted as a very simple and commonplace matter."

* * *

According to Le Roy the intellectual feature of the dogma is not denied nor abrogated. On the contrary it remains in force and takes about the same place in religion as the laws of nature in natural science which formulate uniformities of facts but are not the actual phenomena as experienced. They both have their positive significance. It seems to we that in this way this conception of the dogma is helpful to educated people.

It is not necessary to make the interpretation of religion become a product of the Aristotelian philosophy. It would change theology into an *ancilla* of medieval thinking and deprive it of the liberty to adopt the scientific spirit.

While Le Roy's theory resembles pragmatism, one cannot characterize it as purely pragmatic, and we should consider that the papal decree, *Lamentabile sane exitu* of July 3, 1907, condemns the views of those who claim that dogma is exclusively a *regula praeceptiva actionis*, and that it is not a *regula fidei*. Nor is Le Roy an agnostic. He positively affirms that we can know God in relation to ourselves, and also that we can know him as he is *in se*. The essence of the dogma according to him is not exhausted in its moral significance, but includes also the *enunciatio speculativa*.

The distinction between the actionists and analogists is more one of words than of actual meaning, for both agree in presenting the truth concerning God in terms of intellectual conception and in terms of action, and thus both sides insist on a real cognition of God, each in his own terms. The whole controversy turns on this question, "Is practical truth contained in the speculative, or the spculative in the practical?" while we might say they are both two phases of the same. P. C.

THIS title, "What is a Dogma?" is only a simple question and by no means does it promise an answer. It is a question from the philosopher to the theologian calling for an answer from the theologian to the philosopher.

It would indeed be vain to pretend to give here a complete and definite answer to this complex question. Such problems cannot be solved in a few pages. Therefore the reader must not look for a settled doctrine in the short article which is to follow, nor even for categorical theses on any point. If he sometimes find that I speak in too affirmative a tone let him be kind enough to admit that I do so only for the sake of greater clearness in my questions. In fact I wish to confine myself to simple suggestions which I present merely as rough drafts of solutions offered for the criticism of those who have authority to judge of the subject. And moreover I can justify this attitude of mine by an imperative reason, namely that I am not a theologian and do not like to decide matters in which I am not proficient.

Perhaps some one will ask, why then do I take the trouble to treat a subject of which I admit I have no particular knowledge? Here is my reason. In our day every layman is called upon to fulfil the duty of apostleship in the incredulous world in which he lives. He alone can serve efficiently as the vehicle and intermediary of the Christian message to those who would not trust the priests. Therefore it is inevitable that some problems of apologetics should be laid before him, problems whose solution is an absolute necessity for him if he does not wish to fail in the task which the force of circumstances has laid upon him without possibility of escape, if he wishes to be always ready, following the counsel of the Apostle, to satisfy those who ask him the reason for his faith. It is only natural therefore that I desire to be informed; and if I formulate my question publicly it is because I am not the only one in this situation, and because there is a general interest that the answer shall also be a public one.

Besides I have another motive for acting as I am. If I freely acknowledge my incompetence in a matter which is properly theological, yet on the other hand I consider that I am well situated to appreciate correctly the state of mind in contemporary philosophers that is opposed to the understanding of Christian truth. And it is to this that I bear witness in saying frankly, even brutally (if I must in order to be fully understood), what I know, what I have observed, what perhaps are not always sufficiently comprehended, namely the exact reasons why unbelieving philosophers of to-day repulse the truth that is brought to them, and the legitimate causes (agreeing in this with the Christian philosophers themselves) why they are not satisfied with the explanations that are furnished them.

My ambition goes no farther than to point out certain opinions, perhaps to suggest certain reflections, especially to particularize the statement of certain problems. If the present work bring a useful contribution to the studies of religious philosophy, if it furnish documents and materials which others can turn to account, I shall have attained my end. It is not a question of upholding a system nor of aligning arguments for or against this or that school, but only of elucidating certain fundamental ideas whose consideration is imposed upon every system and upon every school. An effort toward light in the bosom of Catholic truth, faithfully accepted in its completeness and rigor—this is what I submit to the decision of those who have been charged with the duty of defining and interpreting it.

What I desire above all, I repeat, is to make better known the state of mind of those contemporaries who think, the nature of the questions they ask themselves, the obstacles that hinder them and the difficulties that perplex them. It cannot be denied that the classical replies no longer satisfy them; there is no use in disputing over so obvious a fact. The experience of cultivated non-Christian circles (I might even say a personal experience) has demonstrated to me that the proofs brought forward as traditional have no effect on intellects accustomed to the discipline of contemporary science and philosophy. Now why this new impotence of old methods which have sufficed so long? The reason appears to me to be, at least in great measure, that the old apologetics assumes the greater part of the problems to be solved in advance which the moderns, on the other hand, judge to be essential and primordial. The real difficulty for the moderns comes in altogether before the arguments begin by which the theologians flatter themselves they can convince them; it lies in the postulates taken for granted and in the very manner in which the investigation is approached.

It will be well to see how the questions ought to be put to-day; this should be the first result to be obtained. It is the chief result, for without it we would never arrive at anything serious. Thus is imposed the preliminary task of coming in contact with the minds whom one wishes to address and whom one claims to understand. It is necessary that the various chapters of the apologetic should be taken up successively from this point of view in order to be brought to general attention; and in examining here the idea of dogma[1] I only give a first example of the kind of work that I think ought to be generally undertaken.

Let no one think such a task profitless or superfluous. On the contrary, nothing is of greater urgency to-day nor of more pressing necessity. It is strange and lamentable how little we on the Catholic side know or how greatly we fail to appreciate the state of mind of the opponents to whom we try to speak.[2] Nor are we listened to or understood. What we say has no response and carries no weight. We exert ourselves in silence and in a void without even giving rise to any criticism or refutation. In short we only reach those who do not need to be reached—I mean those who are convinced beforehand or whose difficulties are not of a theoretical kind. We must not deceive ourselves. Catholic thought at the present day is without notable influence on the various intellectual movements which are developing around us. It sometimes follows them at a distance and after having resisted them for a long time; but nowhere does it appear capable of directing them, much less of promoting them. There is nothing more sad than to confess so many efforts expended without result on the one hand, and on the other hand so many sincere questions asked which remain unanswered.

Doubtless one might say, and indeed some have said, that there is no need of taking into account modern demands because they proceed from a perverted and misguided judgment. Wretched subterfuge! What contemporaneous thought is asking for beyond what it receives

[1] I will say once for all that by "dogma" I mean especially the "dogmatic proposition," the "dogmatic formula," not at all the reality which underlies it.

[2] I would say the same, moreover, of our opponents with respect to us.

is perfectly legitimate, and there is no justification in pretending to refuse to grant it. Men of to-day are within their rights in not consenting to be held down to the point of view of the thirteenth century. It would indeed be strange if any one should ask for a proof to support a truth of this kind.[3] After all, is it not the very mission and the *raison d'être* of apologetics to address itself to the disordered, if such there be? It must take people as they are and not require of them that they first come of their own accord where it may prefer. Once again, it would be strange if one had no right to make a cure except with certain remedies.

Hence there may be some interest and some profit in the testimony of those whose situation has put them in a position to know the modern mind, its needs and its requirements. These may try to tell how they have come to think what they believe, how they have succeeded in practically overcoming, and of their own accord, the difficulties that they have met like the others. I do not say that we must accept the conclusions of their experiences uncritically; but after all, these experiences offer the advantage of furnishing living documents, not dead opinions, and that is something. I here make no further claim.

One more word before I begin. Perhaps the reader will be surprised to find so long a preamble introducing so short an article. The reason is first of all that I wished to write a sort of general preface for other similar articles intended to follow this one, and also because I wished in this way to forestall any possible misunderstanding. Whatever opinion may be held on the ideas which I shall put forth, it must not happen that any one will try to answer me by charging me with heresy. I affirm nothing in this work except facts easily verifiable by everybody. As to the rest, that is to say the sketches of theories, whatever the form of the language which I have adopted in order to make myself clear, I give them expressly as simple *interrogations* addressed to whomsoever they may concern. In a word, I do nothing but state some problems; it is for the apologists and theologians to solve them.

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[3] The object of faith always remains the same but not the manner of thinking it or of complying with it.

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We no longer live in the day of partial heresies. Formerly a purely logical and dialectic argumentation might suffice because certain common principles were always admitted on both sides. But the case is no longer the same to-day, when these principles go by default, when the fundamental difficulty is to establish a point of departure upon which both sides may agree. To-day denial does not attack one dogma any more than another. It consists above all in a preliminary and total demurrer. The question is not whether a proposition is a dogma or not; it is the very idea of dogma which is repugnant, which gives offense. Why is that?

When we examine the ordinary motives of this repugnance we find four principal ones which I shall briefly enumerate, endeavoring to present them in all their force:

1. A dogma is a statement presented as being neither proved nor provable.[4] Those who declare it to be true declare at the same time that it is impossible ever to arrive at the point of grasping the intimate reasons of its truth. Now modern thought, faithful to the precept of Leibniz, endeavors more and more to demonstrate the old so-called axioms. At least it wishes to justify them with Kant by a critical analysis which shows them to be necessary conditions of consciousness implied *a priori* in every act of reason. It is distrustful of those evidences, pretending to be direct, which were so numerous in former times. Often enough it discovers in them simple postulates adopted for

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[4] I mean here to speak of *intrinsic* proof.

an end of practical utility more or less unconsciously perceived.[5] In short everywhere and always it calls for long and detailed discussions before believing itself authorized to draw conclusions. And it is not just any more or less roundabout proofs that it thus demands, but direct specific proofs. It does not like too general arguments which look upon vast assemblages as a whole and proceed by wholesale demonstrations, because it has had experience too many times with the illusions, mistakes and oversights which they ordinarily conceal. Nor does it like any better external, extrinsic arguments which end in proofs of a negative character, in *reductiones ad absurdum* founded on judgments of contradiction or impossibility, because it has also had experience[6] too many times with their imprudent and hazardous character to declare either impossible or contradictory a thing which may appear so to us only from habit. Therefore it seems that in order to remain faithful to the tendencies which have assured its success in all domains modern thought can do no less than condemn absolutely the very idea of a strictly dogmatic proposition. In what system acceptable to reason could such a proposition find room without violence? Is not the first principle of scientific method incontestably, according to Descartes, that it must hold as true only what clearly appears to be true? What justification would there be for making an exception of just those propositions which pass as the most important, the most profound and the simplest of all? When affirmations are of the greatest consequence and refer to the most difficult and recondite subjects it is certainly not fitting to show oneself less attentive to the exactness of the rules which constitute our protection against error. On the contrary it is just then that it would be legitimate to be even more exacting, more scrupulous, more particular than usual.

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[5] Compare the *Philosophie nouvelle* edition of Bergson's works.

[6] Especially in the sciences.

2. It will doubtless be said that dogmatic propositions are never affirmed without proof. In fact an indirect demonstration has been attempted over and over again. One certain apologetic which is regarded as purely traditional[7] claims to prove that these propositions are true, although it realizes that it is incapable of bringing fully to light the how and the why of their truth. There is some analogy, it seems, between such a proceeding and that of the mathematician who limits himself at times to the theorems of simple existence, or that of the physicist who often accepts facts of which he cannot give any theoretical explanation, or yet again of the historian who always receives knowledge only by the path of testimony. Thus would end the first objection.

Yes, here we would have a very simple solution, but there is one misfortune, namely that the analogy pointed out proves upon reflection to be absolutely inaccurate. The difficulty we wish to avoid reappears *in toto* when we try to justify postulates on which the alleged indirect demonstration rests. When a mathematician is satisfied with establishing a theorem of simple existence, I mean a theorem affirming the existence of a solution inaccessible in itself, he reasons no less rigorously than in other branches of his science. Now here we have nothing like that. It would be necessary to prove *directly* that God exists, that he has spoken, that he has said this and that, that we possess his authentic teachings to-day. This amounts to the same thing as saying that the problem of God, the problem of revelation, of the inspiration of the Bible and of the authority of the church, must be solved by a *direct* analysis. Now these are questions of the same kind as the strictly dogmatic questions, questions with reference to which it is indeed impossible to produce arguments comparable to those of the mathematician. Likewise when a physicist accepts a fact to which he can give no theoretical explanation this fact corresponds, at least for him, to certain definite experiences, to certain manipulations that can be practically carried out, in short to a group of motions of which he has direct knowledge. What similarity is there here? And finally even the historian does not consent to receive truth by testimony except because he is dealing with phenomena of the same kind as those of which he has a direct view by some other means. He still regards his science as always conjectural and uncertain so long as it treats of somewhat profound causes or of events that are more or less remote. How much more ought one draw the same conclusion in the case of dogmas which reflect only facts that are mysterious, strange and disconcerting, and to which no analogy in our human experience corresponds! It has been well done. The alleged indirect proof has inevitably for its basis an appeal to the transcendence of pure authority. It claims[8] to introduce the truth into us fundamentally from the outside in the fashion of a ready-made "thing" which might enter into us forcibly. Thus any dogma whatever seems like a subservience, like a limit to the rights of thought, like a menace of intellectual tyranny, like a shackle and a restriction imposed from without upon the liberty of investigation—all of which is radically opposed to the very life of the spirit, to its need of autonomy and sincerity, to its generative and fundamental principle which is the principle of immanence.

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[7] This method of *extrinsic* demonstration is regarded as traditional. Here is a historical point on which much might be said, but such a discussion is foreign to my subject.

Let us insist a little upon this last point, for the principle of immanence has not always been rightly understood. Too often it has been made out a monster, whereas nothing is more simple nor on the whole more clear. We may say that to have gained a clear consciousness of it is the essential result of modern philosophy. Who refuses to admit it is from that time forth no longer counted among the number of philosophers, who does not succeed in understanding it indicates thereby that he has not the philosophic sense. And this is what constitutes the principle of immanence. Reality is not made of separate pieces put in juxtaposition, but everything is within everything else; in the smallest detail of nature or of science analysis recognizes all of science and all of nature. Each of our states and of our actions comprises our entire soul and the totality of its powers. Thought, in a word, is wholly included in each of its moments or degrees. In short, there is never for us a purely external fact like some sort of raw material. Such a fact indeed would remain absolutely unassimilable, unthinkable; it would be a nonentity to us, for where could we take hold of it? Experience itself is not in the least an acquisition of "things" which previously were entirely unknown to us. No, it is much more a transition from the implicit to the explicit, a profound movement revealing to us the latent requirements and actual abundance in the system of knowledge already explained, an effort of organic development, putting to use its reserves or arousing needs which increase our activity. Thus no truth ever enters into us except as it is postulated by that which precedes it as a more or less necessary complement; just as an article of food to become valuable as nourishment presupposes in the one who receives it certain preliminary dispositions and preparations, for instance, the appeal of hunger and the ability to digest. In the same way the statement of a scientific fact presents this character, no fact having meaning nor, consequently, existing for us except by a theory in which it is born.

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[8] Or at least appears to claim, which is the form under which it is too often presented.

On these various points a critical examination of the sciences has recently come to confirm the reflection of the philosophers. It is obvious that I could not enter here into detail,[9] but the little that I have said will doubtless suffice to give a glimpse at least of how that which has been called *extrinsicism*[10] is opposed in spirit, attitude and method to modern thought.

3. In spite of what we have just said let us admit, however, the instruction of dogmas by simple affirmation of a doctrinal authority which is accepted almost without criticism. Nevertheless, in order to be acceptable these dogmas would need to be perfectly intelligible in their statements, leaving no room for any ambiguity of interpretation or any possibility of error with regard to their real meaning. Now this is not the case. In the first place their formulas often belong to the language of a particular philosophical system which is not always easily understood, which does not always escape the danger of equivocation or even of contradiction. There is no doubt, for instance, that the doctrine of the Word in origin and context is closely connected with Alexandrian neo-Platonism; that the theory of substance and form in the sacraments and that of the relations between substance and accidents in the dogma of the real presence, are really closely connected with Aristotelian and scholastic conceptions. Now these diverse philosophies are sometimes doubtful as to their basis and obscure as to their expression. In any event they have long been antiquated, fallen into disuse among philosophers and scholars. Would it therefore be necessary, in order to be Christians, to commence by being converted to these philosophies? This would be a difficult undertaking, before which

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[9] See the *Bulletin de la société française de philosophie,* meeting of February 25, 1904.

[10] Blondel uses the term *extrincésisme* together with *historicisme* to denote two kinds of apologetics which he condemns. See his article on "Histoire et dogme" in *La Quinzaine* of Jan. 15, Feb. 1 and Feb. 15 of 1904.

many believers themselves would feel strangely embarrassed. And moreover even this would not suffice, for the confusion of many languages resulting from heterogeneous philosophies constitutes still another difficulty no less troublesome than the first.

But this is not all. Aside from this, dogmatic formulas contain metaphors borrowed from every-day matters, for instance when they speak of the Divine Fatherhood or Sonship. It is impossible to give an exact intellectual interpretation of these metaphors, and consequently to determine their precise theoretical value. They are images which cannot be converted into concepts. It would require anthropomorphism to take them literally, and at the same time it would be difficult to give them any deep significance. One cannot even handle them without reserve, nor follow them to a conclusion without arriving too quickly at ridiculous consequences and absurdities. Hence arises a great uncertainty that continues to increase the confusion of imaginative symbols with the abstract formulas of which we were just speaking.

After all, the first difficulty with regard to dogmas which many people find to-day consists in the fact that they do not succeed in discovering a thinkable meaning in them. These statements tell them nothing, or rather seem to them to be indissolubly connected with a state of mind which they no longer possess and to which they think they are no longer able to return without degenerating. Moreover many believers are virtually of the same opinion, and prefer to refrain from all reflection, foreseeing certain obstacles that they would meet in thinking what they believe under the forms laid before them. A contemporaneous philosopher has said: "What would most embarrass the greater number of believers would be if, before asking them for a *proof* of what they believe, one were simply to call upon them to *define* exactly what it is they *affirm* and what they *deny*."[11]

4. Finally, let us pass over these difficulties. Even after they are disposed of there still remains a last objection which seems very grave, namely that in any event dogmas form a group incommensurable with the whole of positive knowledge. Neither by their content nor by their logical nature do they belong to the same system of knowledge as other propositions. They therefore could not be arranged with others in a way to form a coherent system, so that if one accepts them the result is an inevitable breach of unity in the mind, a disastrous necessity of playing a double part. Being unalterable they appear foreign to progress, which is the very essence of thought. Being transcendent they exist without relation to effective intellectual life. They bring no increase of light to any of the problems which occupy science and philosophy. Thus the least reproach that one can cast upon them is that they seem to be without profit, to be useless and barren—a very grave reproach in a period when it becomes more and more perceptible that the value of a truth is measured above all by the services that it renders, by the new results that it suggests, by the consequences which it brings forth, in short by the vivifying influence it exerts on the entire body of knowledge.

Such, briefly summed up, are the principal reasons why the idea of any dogma whatever is repugnant to modern thought. I have endeavored to present them in all their force, taking the same point of view in setting them forth as those who regard them as conclusive and speaking, so to say, not in my own name but in theirs. It remains now to investigate some conclusions and some lessons which we ought to be able to derive from them.

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[11] Belot, *Bibliothèque du congrès international de philosophie de 1900.* Paris: Armand Colin.

These reasons, it must be recognized, are perfectly valid. I do not see any legitimate way of refuting the preceding line of argument.[12] The principles which it invokes seem to me no more contestable than the deductions which it draws from them. In fact I do not see that it has ever been answered except by worthless subtleties or rhetorical artifices.[13] But eloquence is not a proof, neither is diplomacy. Hence our only real resource is to prove that the idea of dogma which is condemned and rejected by modern thought, is *not* the Catholic idea of dogma.

Perhaps it will be found that in speaking in this way I depart from the role in which I have promised to confine myself, that this time decidedly I am stating theses and not asking questions. This would be a mistake. There is no doubt that I am affirming something here, but what? Nothing but *facts*. It is a fact that the unbelievers of to-day are halted in the face of dogmas by the foregoing objections. It is also a fact that whoever (even among believers) has truly comprehended the spirit and the methods of contemporary science and philosophy, cannot but give his assent to these objections. Now please note: those very people who submit most completely and most cordially to the authority established over them could not be affected by it. No authority indeed could bring it about or prevent that I find an argument valid or weak, nor especially that this or that notion has or has not any meaning for me. I not only say that no authority has any right in the world to do so, but that it is absolutely impossible; for after all it is I who do the thinking and not the authority that thinks for me. No argument could prevail against this fact. I can neither force myself to feel satisfaction nor prevent myself from feeling it at the evidence on one side or another. To be sure I admit that authority imposes upon me this or that belief with the result that it makes me follow this or that line of conduct, but how could it compel me by virtue of such a proof to believe what I do not regard as convincing? And how would I be able to obey it if it commanded me to understand this or that declaration which I did not understand at all? As well might it require me to cease thinking. No *reason* can be founded on faith. Here we have an *identity* pure and simple. There is no such thing as *revealed logic*.

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[12] I say refuting, but it could be *cut short* by destroying the postulate which is its root.

[13] It would be interesting to enter into a detailed discussion of these answers, but there is no room for it here.

Hence I come back to what I said a while ago, and, *speaking as a philosopher,* I declare myself incapable of thinking differently from our adversaries on the above-mentioned points.

Moreover in making this declaration I consider that I am doing nothing but stating a problem. The state of mind which I have described exists, it is triumphant to-day; even those who believe the most firmly share it. These are the *facts* which it is impossible not to take into account and which constitute, I repeat, the statement of a question to be solved. Let us see exactly what this question is.

I shall henceforth regard it as granted that the objections summed up above cannot be evaded so long as the idea of dogma which they contain is preserved. Does this mean that we must conclude definitely that there is an absolute incompatibility between the idea of dogma and the essential conditions of reasonable thought? That in order to think as a Christian it is necessary to cease thinking altogether? I certainly do not believe so. But to avoid the objections in the case and to obtain the desired harmony I ask myself if it is not the very manner in which the idea of dogma is presented that is the real cause of the contention, and if consequently we have not reason to change this manner.[14]

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[14] I beg the reader to give heed to the limits within which this question is

Now when we examine the conception of dogma which the four objections above enumerated assume and imply, we are surprised to find that it is common to the greater number of Catholics and their opponents. It is a distinctly intellectual conception. It regards the practical and moral meaning of the dogma as secondary and derived and places in the first rank its intellectual meaning, believing that this constitutes the dogma whereas the other is merely a consequence of it. In a word, it makes of a dogma something like the statement of a theorem—an intangible statement of an undemonstrable theorem, but a statement having nevertheless a speculative and theoretical character and relating above all to pure knowledge. This is the common postulate that one discovers by analysis at the foundation of both of the two opposed doctrines, the one that accepts and the one that rejects the idea of dogma. Here I believe is the crux of the difficulty. From this unexpressed postulate and from the conception which flows from it originate, in my opinion, both the abuses to which the idea of dogma can give rise and the conscientious objections that it raises. Indeed it is inevitable that one would finally draw the conclusion that all dogma was illegitimate, for he would at the same time define it as a theoretical statement while nevertheless attributing to it characteristics the very opposite of those which make statements correct. It is very curious that the apologists are not more often informed of a fact of such great importance as that their conception of dogma would destroy in advance the theses that they wish to establish. On the other hand, the same intellectualist idea of dogma leads to two very regrettable and unfortunately very frequent exaggerations; one consists of confusing dogmas properly so called with certain opinions

comprised. It does not discuss in any way the modification of the content of dogma, nor even its traditional religious interpretation, but only the determination of the modality of the dogmatic judgment and of the qualification it possesses.

and certain theological systems, that is to say, with intellectual accessory representations; the other, in failing to see that a dogma could never possess any scientific significance and that there are no more dogmas concerning for instance biological evolution than there are concerning the movements of planets or the compressibility of gas.

From a thorough study of these various points we reach the conviction that the problem of dogma is usually badly stated;[15] and perhaps we will see at the same time how it ought to be stated in order to render possible a satisfactory solution.

* * *

From this point I enter at once into the domain in which I must keep myself in an interrogatory attitude. This is my definite intention although to insure clearness I may keep the didactic tone. What follows must be taken as a simple exposition of what I ordinarily reply to those who ask me what I think of the idea of dogma. Am I wrong to speak in this way? I am quite ready to acknowledge it if any one will show me that it is not the right way in the eyes of the church.

First of all I say that a dogma cannot be compared to a theorem, of which we only know the statement without its proof and whose proof can only be guaranteed by the assertion of a teacher. Nevertheless I know that this is the most common conception. We like to think of God in the act of revelation as a very wise professor whose word we must believe when he communicates to his audience results whose proof that audience is not capable of understanding. But this appears to me to be hardly satisfactory. We say that God has spoken. What does the word "speak" mean in this case? Most certainly it is a metaphor. What is the reality which it conceals? Herein lies the whole difficulty.

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[15] At least in books in current use and in elementary education.

Without recurring to the general considerations I have already developed let us take some examples that will serve to specify what we have hitherto looked upon only in large outlines.

"God is a person." Here we have a dogma. Let us try to see in it a statement having above all an intellectual meaning and a speculative interest, a proposition belonging first of all to the order of theoretical knowledge. I pass over the difficulties aroused by the word "God," but let us consider the word "person." How must we understand it?

If we grant that the use of this word bids us conceive the divine personality in the form shown to us by psychological experience on the model of what common sense designates by the same name, as a human personality, idealized and carried on to perfection, we have here a complete anthropomorphism, and Catholics would certainly agree with their opponents in rejecting such a conception. Moreover to carry such a thought to its extreme limits is a very delicate thing, very likely to induce error or at least mere verbiage, incapable in any event of producing anything more than very vague metaphors and perhaps even eventually contradictory results.

Shall we limit ourselves to saying that the divine personality is essentially incomparable and transcendent? Very well, but if so it is very badly named, and in a way which seems made expressly to induce delusion. For if we declare that the divine personality does not resemble in any respect that with which we are acquainted, what right have we to call it "personality"? Logically it should be designated by a word which would belong only to God, which could not be employed in any other instance. This word would therefore be intrinsically undefinable. Let us imagine any assemblage whatever of syllables deprived of all possible significance. Let A be this assemblage. Then by our hypothesis "God is a person" does not have any other meaning than "God is A." Is this an idea?

The dilemma is unsolvable for any one who is seeking an intellectualist interpretation of the dogma "God is a person." Either he will define the word "personality," and then he is fatally sure to fall into anthropomorphism; or he will not define it, and then he will fall none the less fatally into agnosticism. Here we have a circle.

The same remarks hold with regard to the propositions "God is conscious of himself; God loves, wills, thinks, etc."

Let us take another example, the resurrection of Jesus. If this dogma, whatever may be eventually its practical consequences, has for its first aim to increase our knowledge in guaranteeing to us the accuracy of a certain fact, if it constitutes before all a statement of an intellectual character, the question to which it first gives rise is this: What precise meaning does it assume is to be attached to the word "resurrection"? Jesus, after having experienced death, has once more become alive. What does this mean from the theoretical point of view? Doubtless nothing except that after three days Jesus reappeared in a state identical with that in which he was before he was nailed on the cross. Now the Gospel itself tells us exactly the opposite. The resurrected Jesus was no longer subject to ordinary physical or physiological laws; his "glorified" body was no longer perceptible in the same conditions as before, etc. What does this mean? The idea of life has not the same content when applied to the period preceding the crucifixion as to that which followed it. Now what does the word represent with relation to this second period? Nothing that can be expressed by concepts. It is simply a metaphor which cannot be converted into specific ideas. Here again, to be exact, it would be necessary to create a new word, a word reserved for this single case, a word consequently to which it would not be possible to give any regular definition.

Let us borrow a final example from the dogma of the real presence. Here it is the term "presence" which must be interpreted. What does it usually signify? A being is said to be present when he is perceptible, or when though he himself cannot be grasped by perception he yet manifests himself by perceptible effects. Now according to the dogma itself neither of these two circumstances is realized in the case in hand. The presence in question is a mysterious presence, ineffable, unique, without analogy to anything that one ordinarily understands by that name. Now I ask, what idea is there here for us? A thing that can neither be analyzed nor even defined could not be called an "idea" except by an abuse of the word. We wish a dogma to be a statement of an intellectual order. What does it state? It is impossible to say exactly. Does not this fact condemn the hypothesis?

Finally the pretension of conceiving dogmas as statements whose first function would be to communicate certain theoretical bits of knowledge would run against impossibilities on every hand. It seems to end inevitably in reducing dogmas to pure nonsense. Perhaps it must for this reason be resolutely abandoned. Let us therefore see what different kind of significance remains possible and legitimate.

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First of all, if I do not deceive myself, a dogma has a *negative* meaning. It excludes and condemns certain errors instead of positively determining the truth.[16]

Let us once more take up our former examples. We shall first consider the dogma "God is a person." I nowhere see in it any definition of the divine personality. It teaches me nothing about that personality. It does not reveal its nature to me nor furnish me with any explicit idea. But I see clearly that it tells me, "God is not impersonal"; that is to say, God is not simply a law, a formal category, an ideal principle, an abstract entity, any more than he is a universal substance, or some unknown cosmic force diffused throughout the world. In short, the dogma "God is a person" does not bring to me any new positive conception nor does it any more guarantee to me the truth of any particular system among those which the history of philosophy shows to have been successively proposed, but it warns me that this or that form of pantheism is false and ought to be rejected.

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[16] We shall shortly see how dogmas are more and greater than this. But at the start I shall place myself in a strictly intellectualist point of view.

I would say the same with regard to the real presence. The dogma does not tell me any theory about that presence, it does not even teach me in what it consists; but it tells me very clearly that it must not be understood in such or such a way as were formerly proposed, that for instance the consecrated host must not be regarded solely as a symbol or a figure of Jesus.

The resurrection of Christ gives rise to the same remarks. This dogma does not teach me in any degree what was the mechanism of this unique fact nor of what kind the second life of Jesus was. In short it does not communicate a conception to me. But on the contrary it excludes certain conceptions that I might be tempted to make. Death has not put an end to the activity of Jesus with reference to the things of this world. He still mediates and lives among us, and not at all merely as a thinker who has disappeared and left behind a rich and living influence and whose work has left results through the ages; he is literally our contemporary. In short, death has not been for him, as it is for ordinary mortals, the definite cessation of practical activity. This is what the dogma of the resurrection teaches us.

Shall I insist further? It does not seem advisable at this time. The foregoing examples are sufficient to make the principle of interpretation that I have in mind clearly understood. Of course long expositions would be necessary if we would enumerate in detail all the consequences of this principle and all its possible applications, and an enumerative study of the different dogmas would therefore become indispensable. But this is not my real purpose. I wish to confine myself simply to indicating an ideal. This is why I do not undertake either to multiply examples or even to develop any one of them completely.

Moreover the idea is not a new one. It belongs to the most authentic tradition. Is it not indeed the classical teaching of theologians and scholars that in supernatural matters the surest method of investigation is the *via negationis*? Permit me to recall in this connection a well-known text of St. Thomas: "But the *via remotionis* is to be used chiefly in considering divine substance. For divine substance by its immensity exceeds every form which our mind can touch; and so we cannot grasp it by knowing what it is, but some sort of a notion of it we have by knowing what it is not."[17]

Nevertheless I ought to point out one objection which might occur to the mind. We will easily grant that the dogmatic formulation promulgated by the church in the course of history has especially a negative character, at least when looked upon from an intellectual point of view as we are doing at this time. In fact, the church itself declares that its mission is not in the least to produce new revelations but only to maintain the *depositum revelationis*, and the negative method here adopted is entirely suitable for this mission. And yet, of what does this *depositum* consist if not of a certain collection of original affirmations? Take the primary expression of Christian faith, the Credo. What could be more positive? Now here is the basis of doctrine, that which characterizes and constitutes it. Moreover when we say "revelation" we certainly say affirmation and not negation.

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[17] "Est autem via remotionis utendum praecipue in consideratione divinae substantiae. Nam divina substantia omnem formam, quam intellectus noster attingit, sua immensitate excedit; et sic ipsam apprehendere non possumus cognoscendo quid est, sed aliqualem ejus habemus notitiam cognoscendo quod non est."—*Contra Gentiles*, I, xiv.

Certainly we do. I do not contradict it in the least, but we must make a distinction. The creed of Nicaea and Constantinople contains many traces of a negative dogmatic elaboration: for instance, on the divinity of Christ as against the Arian heresy; on the procession of the Holy Ghost in opposition to the Macedonians, etc.[18] Consequently there is nothing on this head to contradict our conclusions. It is only the grammatical form which is affirmative here; in reality we are treating of errors to be excluded rather than theories to be formulated. But let us take the Apostles' Creed. Here indeed we have nothing negative but neither do we have anything properly intellectual and theoretical, nothing which belongs properly to the order of speculative knowledge, nothing in short which resembles the statement of theorems. It is a profession of faith, a declaration of attitude. We shall soon examine dogmas from this practical point of view (which I hasten to say is in my eyes the principal point of view), yet we shall stop a moment at the intellectual point of view. The Apostolic Creed in its original form affirms the existence of realities of which it gives not even a rudimentary representative theory, hence its only role with reference to abstract and reflective knowledge is *to state objects and therefore problems*. Finally we see that the proposed objection is not valid and we can maintain our thesis until further notice.

Thus in so far as they are statements of a theoretical order dogmas have all a negative meaning. History proves this when it procures our assistance at the birth of one after another of them in relation to the several heresies.[19] The rise of all dogmas has always followed the same course, has always presented the same phases: at the beginning purely human speculations, some explanatory systems very similar to other philosophical systems, in short, attempts at theories relating to religious facts, to mysterious realities experienced by Christendom in its practical faith; then only come the dogmas for the purpose of condemning certain of these attempts, of taxing certain of these conceptions with error and of excluding certain of these intellectual representations. Hence it follows that dogmatic formulas often borrow expressions from different philosophies without taking the trouble to fuse together and unify these heterogeneous languages.

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[18] It would be easy to insist on the example of *consubstantialem* or of *Filioque*.

This offers no more disadvantages than does the use of concepts derived from different origins, from the moment that dogmas do not tend to constitute by themselves a rational theory, an intelligible system of positive affirmations, but confine themselves to opposing certain exceptions to certain hypotheses and conjectures of the human mind. On the other hand it is natural that each dogma should put itself in the point of view belonging to the doctrine that it lays under an interdict, in order to attack it directly without danger of ambiguity. Hence it also follows that dogmatic formulas can enact laws on the incomparable and the transcendent and yet not fall into the contradictions of anthropomorphism or of agnosticism. It is man who with his opinions, his theories and systems, gives to dogmas their intelligible substance;[20] these are confined to pronouncing a veto at times, to declaring at times that

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[19] Compare the usual formula of the decrees of council: "*Si quis dixerit. ..., anathema sit.*"

[20] From the theoretical point of view, understand. Dogmas are thought in terms of the human systems which they oppose. [This view is recently endorsed by Catholic theologians of such recognized authority as Cardinal Billot. S. J.—Tr.]

"such an opinion, such a theory, such a system, is not allowed," without ever pointing out why they should not be accepted, nor by what they must be replaced. Thus negative dogmatic definitions do not limit knowledge nor put an end to progress; in short they only close up false paths.

From the strictly intellectual point of view it seeems to me that dogmas have only the negative and prohibitive sense of which I speak. If they formulated absolute truth in adequate terms (to assume that such a fiction has a meaning) they would be unintelligible to us. If they gave only an imperfect truth, relative and mutable, they would not be justified in obtruding themselves. The only radical way to put an end to all the objections on principle against dogma is to conceive of them, as we have already said, as being undefinable in so far as they are speculative propositions, except with relation to previous doctrines upon which they promulgate an unwarranted judgment. Moreover is it not the teaching of theologians, including the most intellectualist, that in a dogmatic statement the reasons which can be incorporated in the text are not in themselves objects of faith imposed upon belief?

There is one important consequence resulting from the foregoing, namely, that the true method of studying dogmas (from the intellectual point of view, understand) is the historical method. The science known as positive theology, or rather the history of dogma, seeks to perform this task. The method has an effective apologetic value much greater than purely dialectic dissertations. Because in any event it is impossible to comprehend dogmatic statements, there is the greater reason for justifying them if one would commence by plunging them once more into their natural historical environment without which their authentic meaning becomes more and more vague and finally ends by vanishing entirely.

Nevertheless dogmas do not have merely a negative meaning, and even the negative meaning that they offer when regarded from a certain direction does not constitute their essential and primary significance. This is true because they are not merely propositions of a theoretical character, because they must not be examined solely from the intellectual point of view, from the point of view of knowledge. This is what we shall now elucidate further.

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Here more than ever I insist that the intention and tendency of the pages to follow must not be misunderstood. I repeat that the affirmative tone is used only as a means for clearness. At bottom the question is always the same as I specified at the beginning. Here, if I may say so, is the form in which experience has shown me that the notion of dogma is most easily assimilable to the minds of to-day:

A dogma has above all a *practical* meaning. It states before all a prescription of a practical kind. It is more than all the formula of a rule of practical conduct. This is its principal value, this its positive significance. This does not mean, however, that it must be without relation to thought, for (1) there are also certain duties concerned with the act of thought; (2) it is virtually affirmed by the dogma itself that under one form or another reality contains wherewith to justify the prescribed conduct as reasonable and wholesome.

I take pleasure in quoting in this connection the following passage from R. P. Laberthonnière:[21] "Dogmas are not simply enigmatical and obscure formulas which God has promulgated in the name of his omnipotence to mortify the pride of our spirits. They have a moral and practical meaning; they have a vital meaning more or less accessible to us according to the degree of spirituality we possess."

After all, when converts, in spite of good intentions, themselves create part of the theoretical difficulties under discussion do we not answer them daily: "Never mind all that, it is not important. Do not believe that God requires so many formalities. Come to him fairly, frankly, simply, according to the wise words of Bossuet. Religion is not so much an intellectual adherence to a system of speculative propositions as it is a living participation in mysterious realities." Why not then make theory agree with practice?

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[21] *Essais de philosophie religieuse*, p. 272. Paris: Lethielleux.

Let us keep the same examples. They represent well enough the different types of dogmas. "God is a person" means, "Conduct yourself in your relations to God as in your relations with a human person." Likewise "Jesus has risen" means, "Be in relation to him as you would have been before his death, as you are with a contemporary." In the same way again the dogma of the real presence means that one must have the same attitude toward the consecrated host as one would have toward Jesus had he become visible, and so on. It would be easy to multiply these examples, and also to develop each of them farther.[22]

That dogmas can and ought to be interpreted in this way there is no doubt, and the fact will not be contested by any one. In fact, it cannot be repeated too often that Christianity is not a system of speculative philosophy but a source and regimen of life, a discipline of moral and religious action, in short the sum total of practical means to obtain salvation. What then is surprising in the fact that its dogmas primarily concern conduct rather than pure reflective knowledge?[23]

I do not think it is necessary to insist farther upon this point, but I wish to indicate in a few brief words the most important consequences of the principle here laid down.

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[22] I do not claim in the least that the foregoing comments exhaust the meaning of the dogmas mentioned: they will suffice to point out a line of inquiry.

[23] This is why assent to dogmas is always a free act and not the inevitable result of a compelling line of argument.

First of all it is clear that the general objections summed up at the beginning of this article do not affect this conception of dogma to the same extent and in the same degree as they do the usual intellectualist conception, for that provokes the conflict and renders the difficulty insurmountable, whereas on the other hand we may now catch a glimpse of a possible solution. As there is no question of obtaining a theoretical statement in conditions radically opposed to those prescribed by scientific method, we no longer find ourselves face to face with a logical stumbling-block but only with a problem referring to relations between thought and action—a difficult problem certainly, but not unapproachable and one which at any rate does not appear absurd after it is stated.

Of course there are always important questions to be solved. It is necessary to supply the dogma in some way with a demonstration and justification, and this is by no means a perfectly easy matter. Nevertheless one of the greatest obstacles has been smoothed away. Practical truths are established differently from speculative truths. Recourse to authority which is entirely inadmissible in the realm of pure thought seems *a priori* less shocking in the domain of action, because if authority has legitimate rights anywhere it certainly has in the domain of practical affairs.

The Council of the Vatican tells us: "If any shall say that no true mysteries properly so-called are contained in divine revelation, but that all the dogmas of faith can be comprehended and demonstrated through reason duly perfected by natural principles, let him be anathema."[24] Now if faith in dogmas were first of all knowledge, an adherence to some statements of an intellectual kind, one could not comprehend either that assent to unsolvable mysteries could ever be legitimate or even simply possible, or in what it might consist, or what sort of utility or value it might have for us, or how it might constitute a virtue. On the other hand all this can be understood if faith in dogmas is a practical submission to commandments which have to do with action. Nothing is more normal than activity placing mysteries before intelligence.[25]

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[24] "Si quis dixerit in revelatione divina nulla vera et proprie dicta mysteria contineri, sed universa fidei dogmata posse per rationem rite excultam a naturalibus principiis intelligi et demonstrari, anathema sit."

The Council of the Vatican tells us further: "If any shall say that assent to the Christian faith is not free.... let him be anathema."[26] This text is generally explained by recognizing that the reasons for believing, the motives of credibility, are not of insuperable force, a mathematical evidence, and that in consequence a decisive act of the will or of the heart is always necessary to conclude the investigation definitely. Is this not virtually admitting that one cannot see in belief in dogmas an act which should first of all be intellectual without making it thereby inferior to the ordinary acts of thought? How would such an act—an act performed under conditions contrary to the nature of thought—be even legitimate or merely possible? But on the other hand it is easy to believe that the practical acceptance of commandments relating to action depends on our free will and gains in perfection by not being able to manifest itself by necessary consequence. Let us insist a little upon this point, for it is of highest importance in the problem of the relations between reason and faith.

From the beginning apologetics is confronted with a grave difficulty which perhaps cannot always be satisfactorily disposed of. On the one hand it is clearly understood that an act of faith is a free act and that its object, as well as its supreme motive, is supernatural. But on the other hand an act of reason ought to precede and prepare the act of faith, for it is reason alone by which the obligation and necessity of overreaching reason can be recognized. And an act of reason must also constantly accompany the act of faith, for it is necessary that the human mind shall have some sort of hold upon the dogma if it wishes to accept it. St. Thomas said well: "Those things which are under the faith. . . .no one would believe unless he sees they ought to be believed."[27]

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[25] Submission to dogmas then from one point of view is for the believer what submission to facts is for the scholar.

[26] "*Si quis dixerit assensum fidei Christianae non esse liberum...., anathema sit.*"

Now how shall we reconcile these two opposite requirements in a system of intellectualist interpretation? Either we would maintain (as there are some who do) that the apologetic proofs are absolutely positive and exact; and then what would become of the liberty of the act of faith? Or in order to safeguard that liberty we would call them insufficient and only more or less probable; and then our faith would lack any basis, for after all an insufficient proof is not an acceptable proof, especially in so important and difficult a matter. An intellectualist attitude becomes disarmed in the face of this dilemma since liberty does not belong to the domain of pure intelligence and has no place or part in the proceedings of discursive reason. But with the other attitude the dilemma can be solved because this time the dialectic in the case is action and life not simply argument, and liberty revives with life and action.

Likewise we have here the objection relating to the intelligibility of dogmatic formulas. Although these formulas are hopelessly obscure, even inconceivable, when we want them to furnish positive determinations of truth from a speculative and theoretical point of view, they nevertheless show themselves capable of clearness if we are careful not to ask of them anything but instruction as to practical conduct. What difficulty, for instance, do we find in understanding the dogmas of the divine personality, of the real presence, or the resurrection in the practical system of interpretation just outlined? Although these dogmas are mysteries for the intelligence that demands explanatory theories they are nevertheless susceptible of perfectly clear statement as to what they prescribe for our actions. Hence the language of common sense has its place as well as the use of anthropomorphic symbols and the employment of analogies or metaphors, and neither the one nor the other gives rise to unsolvable complications since this time it is a question only of propositions relating to man and his attitudes.

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[27] "Ea quae subsunt fidei. . . . nemo crederet nisi videret ea esse credenda."

We also see now what the relation is between dogmas and efficient life. We predict for them a possibility of experimental study and of gradual research which has heretofore escaped us. Finally we understand how they can be common to all, accessible to all, in spite of the inequality between intellects, whereas to conceive them in the intellectualist way one would be inevitably led to make a distinction of an intellectual aristocracy. I have not room here to develop these different considerations as much as I should like, but I imagine that a simple indication after all may be sufficient for the time being, and that the reader can carry the process on for himself without any difficulty. Nevertheless it seems necessary to me to prevent a possible objection in order to avoid all misapprehension.

I have spoken of *practice*. This word must be rightly understood. I take it in the widest acceptation of the term. *Action* and *life* are here synonymous. Hence the word does not in the least mean a blind step, without relation to thought or consciousness. In fact there is an act of thought which accompanies all our actions, a life of thought which mingles throughout our life; in other words, to know is a function of life, a practical act in its way. This function, this act, is also called *experience,* a name which indicates at the same time that we are not at all dealing with actions performed without any sort of light but that the light in question is not that of simple argumentative reason.

I have also spoken of the activity which places mysteries before intelligence, and by way of elucidation I have cited the example of scientific facts. To comprehend what I mean by this, one must not forget that a scientific fact is not a thing to be submitted to passively. If there is any semblance of a purely external fact, of a mystery totally opaque, of a violent commandment from without, it is so with respect to argumentative understanding. But the thought-action of which I was just now speaking avoids this appearance. It infinitely exceeds the purely intellectual thought. I have not heard anything to affirm otherwise.[28]

Hence there is a necessary relation between dogmas and thought. It is at the same time both a right and a duty not to be content with a blind belief in dogmas but to strive also in proportion to one's strength to think them. The system of separation, of tight partitions, of the twofold accountability of conscience, is not desirable nor, to speak truthfully, possible. It is contrary to the demands of that faith which wishes to hold every man; it is contrary to the requirements of philosophy which desires a spiritual unity; and finally it is contrary to the requirements of morality which cannot approve an action that is systematically unconsidered.

But thought when applied to dogmas should not misunderstand their primarily practical meaning. The path to be followed is the test of practical experience and not an intellectual dialectic. The inspiring principle is perfectly expressed in the sacred word, *qui facit veritatem venit ad lucem.*

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[28] The reader who desires to pursue this point further may refer to several articles I have published since 1889 in the *Revue de métaphysique et de morale*, and in the *Bulletin de la société française de philosophie.*

Thus translated into terms of action the traditional methods of *analogy* and *eminence* assume a very clear significance. Under the guise of metaphors and images they affirm that supernatural reality contains the wherewith to make obligatory by law that our attitude and our conduct with regard to it should have such or such a character. The images and metaphors—which are hopelessly vague and fallacious when one tries to see in them any approximation whatever of impossible concepts—become on the other hand wonderfully illuminating and suggestive after one looks to find in them only a language of action translating truth by its practical echo within ourselves.

It remains finally to specify the relations of dogmas, understood in the way we have described them, to theoretical and speculative thought, to pure knowledge. In what respect do they govern our intellectual life? How does their intangible and transcendent character leave the full liberty of research intact as well as the undeniable right of the mind to repulse every conception which tries to impose itself from without? We shall easily see.

The Catholic is obliged to assent to the dogmas without reservation. But what is thereby imposed upon him is not in the least a theory, an intellectual representation. Such a constraint indeed would inevitably lead to undesirable consequences: (1) The dogmas would in that case be reduced to purely verbal formulas, to simple words whose repetition would constitute a sort of unintelligible command; (2) Moreover these dogmas could not be common to all times nor to all intelligences.[29]

No, dogmas are not at all like that. As we have seen, their meaning is above all practical and moral. The Catholic, obliged to accept them, is not restrained by them except as regards rules of conduct, not as regards any particular conceptions. Nor is he condemned to accept them as simple literal formulas. On the contrary, they offer him a very positive content, explicitly intelligible and comprehensible. I will add that this content, having to do solely with the practical, is not relative to the variable degree of intelligence and knowledge; it remains exactly the same for the scholar and the ignorant man, for the exalted and the lowly, for the ages of advanced civilization and for the races still in barbarism. In short it is independent of the successive states through which human thought passes in its effort toward knowledge, and thus there is *only one faith for everybody.*

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[29] In the two words "esotericism" and "Pharisaism" would be the inevitable double rock upon which they would split.

This being granted, the Catholic after having accepted the dogmas retains full liberty to make for himself whatever theory, or whatever intellectual representation he wishes of the corresponding objects—the divine personality, the real presence, or the resurrection, for instance. It remains with him to grant his preference to the theory which best agrees with his own views, to the intellectual representation which he deems the best. His position in this respect is the same as that toward any scientific or philosophical speculation, and he is free to adopt the same attitude in both cases. Only one thing is imposed upon him, only one obligation is incumbent upon him; his theory must justify the practical rules expressed by the dogma, his intellectual representation must take into account the practical edicts prescribed by the dogma. Thus in a word it appears almost like the statement of a fact with regard to which it is possible to construct many different theories but which every theory must take into account, like the expression of a truth many of whose intellectual representations are legitimate but of which no explanatory system can well be independent.[30]

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[30] It is at this point that we must distinguish between *intellectual formula* and the *underlying reality* in the dogma.

From this naturally follows the step that we have recognized as usual with religious thought in its effort at elaboration. Let us take any dogma whatever, Divine Personality, the real presence or the resurrection of Jesus. By itself and in itself it has only a practical meaning. *But there is a mysterious reality corresponding to it and therefore it presents to the intelligence a theoretical problem.* The human intellect at once takes possession of this problem; and obeying simply and solely the laws of its own nature it imagines the explanations, the answers, the systems codified in the precepts of scientific method and the principles of reason.[31] As long as the theory constructed in this way respects the practical significance of the dogma it is given *carte blanche.* Hence to pass judgment on the theories remains the task of pure human speculation, and any authority exterior to the thought itself has neither the right nor the power to interfere.[32] But once let a theory arise which makes an attack on dogma in its own domain by altering its practical significance, and the dogma would immediately array itself against it and condemn it, thus becoming a negative intellectual statement superimposed upon the rule of conduct which at first it was, purely and simply.

Hence one sees positively how the two meanings of a dogma, the practical meaning and the negative meaning, are reunited, the latter being subordinated to the former. Moreover we see how dogmas are immutable and yet how there is an evolution of dogmas. What remains constant in the dogma is the orientation that it gives to our practical activity, the direction in which it inflects our conduct. But the explanatory theories, the intellectual representations, change constantly in the course of the ages according to individuals and epochs, freed from all the fluctuations and all the aspects of relativity manifested by the history of the human mind. The Christians of the first centuries did not profess the same opinions on the nature and personality of Jesus as we, and they did not have the same problems. The ignorant man to-day does not have at all the same ideas on these lofty and difficult subjects as the philosopher does, nor the same mental preoccupations. But whether ignorant men or philosophers, men of the first or the twentieth century, every Catholic has always had and always will have the same practical attitude with regard to Jesus.

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[31] In this respect the Middle Ages had an independence and a boldness which we have forgotten.

[32] Religious authority which has souls in its charge can indicate certain theories as dangerous, as long as they run the risk of being wrongly understood and thus of reacting injuriously upon conduct. Hence arise censures of an inferior note to those of heresy. But these condemnations are not properly dogmatic.

* * *

It is time to conclude and I will do so in as few and brief words as possible.

Two main results seem to me to have been attained by the foregoing discussion:

1. The intellectualist conception which is current to-day renders the greater number of objections raised by the idea of dogma unsolvable.

2. On the other hand, a doctrine of primacy of action permits a solution of the problem without abandoning either the rights of thought or the requirements of dogma.

If these conclusions were admitted, the apologetics of our days would be under the irresistible necessity of modifying many of its arguments and methods.

Now, can these conclusions be admitted without loss to faith? It is for the theologians to tell us, and in case their response is negative to teach us how they expect otherwise to prepare to surmount the obstacles which perplex us.

EDOUARD LE ROY.

LEIBNIZ IN LONDON.[1]

LEIBNIZ paid two visits to London from Paris, where he was staying from March, 1672, to October, 1676: the first visit, which was in connection with the embassy from the Elector of Mainz, was from January 11 to the beginning of March, 1673; the second was made on his way home to Germany, when he stopped in London for about a week in October, 1676.

Leibniz had a habit of writing out all the important scientific points in the correspondence that he kept up with noted people, so that he might thus impress them the more deeply upon his memory. I have discovered among his manuscripts three folio sheets on which he has written down the things worth noting in connection with these two visits to London.[2] The sheets which relate to his second visit have been known to me for some time; but the other ones, referring to the first visit, I came across only during my last stay in Hanover in the summer vacation of the year 1890.

In what follows, I have only paid attention to the contents of these sheets which refer to mathematics.[3]

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[1] Translated by J. M. Child, from an article by Dr. Gerhardt in the *Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin,* 1891, pp. 157-165. The notes are by the translator.

[2] These highly important documents ought to be photographed and published in facsimile.

[3] It seems a pity that Gerhardt has not given the contents of the section labeled "Mechanica," unless indeed this is all non-mathematical; there may

The sheet relating to Leibniz's first visit to London, of which I have added a partial transcript under the heading I, is divided on both pages into sections [the word used in the original is *Felder* = columns, but it will be seen that, according to the transcript given later, the sections are horizontal and not vertical], in which Leibniz has entered all that he considered to be worth noting. While the sections labeled "Chymica," "Mechanica," "Magnetica," "Botanica," "Anatomica," "Medica," and "Miscellanea" are filled up with an extraordinary number of memoranda, the first sections, which are allotted to mathematical subjects, are very poorly filled. That labeled "Geometrica" contains a note that is especially worth remarking: "Tangents to figures of all kinds. Development of geometrical figures by the motion of a point in a moving straight line."[4] In all probability it may be supposed that this refers to the lectures of Barrow, delivered on his method of tangents at the University of Cambridge down to the year 1669. As is well known, the method of Barrow is only applicable to such curves as can be expressed by rational functions.[5] Newton's name was mentioned in

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be in it some intimation that would lead to a clue as to the origin of Leibniz's use of the word *moment*, meaning thereby, not Newton's use of the word, but the idea now familiar to us in the determination of the center of gravity of an area, expressed by the equation

$$x = \Sigma ax/\Sigma a,$$

where a is the element of the area distant x from the axis, x the distance of the center of gravity from that axis, and Σax is the sum of the 'first moments of the elements' or 'the first moment of the whole area.' See note 16, later.

[4] *"Tangentes omnium figurarum. Figurarum geometricarum explicatio per motum puncti in moto lati."*

[5] In a footnote, Gerhardt asserts that "Barrow's *Lectiones Geometricae* appeared in 1672." This is incorrect; for they were published, combined with the second edition of the *Lectiones Opticæ*, in 1670; nor can Gerhardt be referring to the second edition, for that appeared in 1674 and then as a separate volume. Also, I have, in the little book on *The Geometrical Lectures of Isaac Barrow*, published by the Open Court Publishing Co., given reasons for supposing that these lectures were never delivered as *Lucasian Lectures*, though they may have formed the subject-matter for college lectures at Gresham and Trinity. Again, it is not true, although "well known," that "the method of Barrow was only applicable to such curves as can be expressed by rational functions"; this remark is even only partially true about the differential triangle method; for, as I have shown in the above-mentioned book, Barrow had a complete calculus, which included, among other things, the important idea of *substitu-*

the "Optica." Leibniz has the remark: "They told me about a certain phenomenon that Barrow confessed he was unable to solve. Newton's difficulty has so far not been solved, Father Pardies having given it up."[6] Obviously this remark applies to Newton's experiment on the refraction of light by a prism and to the decomposition of white sunlight, and especially to the fact that a circular solar image becomes after refraction a long spectrum. Father Pardies of Clermont had published in opposition to Newton his "Two Letters containing Animadversions upon I. Newton's Theory of Light," in the *Philosophical Transactions* of 1672, together with a letter from Newton.

It cannot be said for certain that Leibniz, during his first stay in London, met with any of the great English mathematicians; Wallis lived at Oxford, while Barrow and Newton resided at Cambridge.[7] Indeed, it is made a matter of plaint by Brewster, the biographer of Newton, that the Royal Society of London at that time numbered few men of distinguished talents who were in a position to perceive the truth of the optical discoveries of Newton. In the letter which Leibniz addressed to Oldenburg, the Secretary of the Royal Society, during his visit to London, he men-

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tion, which is all that is necessary to complete the "*a*-and-*e*" method and make it applicable to surds and fractions, and probably was thus applied by Barrow in working out his constructions; but the whole thing was geometrical, which apparently hid the inner meaning until recently.

To my mind, the mention of but "tangents and local motion" points out that, on Leibniz's *first* reading of Barrow, he only perused at all carefully the first five lectures, which are relatively unimportant; or rather it confirms an opinion I had already expressed to Mr. P. E. B. Jourdain.

[6] *"Locuti sunt mihi de phaenomeno quodam quod Barrovius fatetur se solvere non posse. Newtoni difficultas soluta hactenus non est, P. Pardies manus dante."*

[7] It seems however that Leibniz attended the meetings of the Royal Society; at any rate once, when he exhibited the model of his calculating machine. It would be interesting if the roll of members present on all occasions during this period could be obtained, as doubtless they were kept. For such men as Ward were members at the time and attended the meetings, and Ward was, if not in the same class as the three whose names are given, an excellent mathematician; and, Leibniz, being somewhat of a notable, on account of his connection with the Embassy from Mainz, would surely be introduced to all eminent members present.

tioned that he had met by accident the mathematician Pell at the house of Boyle, the chemist. The conversation fell upon those number-series which in elementary mathematics were called the higher arithmetical series and whose sums and terms were found by the help of differences. Leibniz showed that he had gone deeply into the study of such series and had partly found out some new methods for calculating the terms.[8] Leibniz's letter to Oldenburg was dated Feb. 3, 1673 (1672 O. S.).[9]

From the preceding it appears that what Leibniz learned with reference to mathematics from his first visit to London was quite unimportant.[10] The chief aim of his stay in London was to be elected as a Fellow of the Royal Society; and this came to pass, owing in part to an exhibition of a model of his calculating machine, and in part to the friendly offices of Oldenburg.

After his return to Paris at the beginning of March, 1673, Leibniz was able to find more leisure to follow up his studies without hindrance; the political mission which was the cause of his being sent to Paris, was now at an end.

It may be regarded as certain that, before his first visit to London, Leibniz made the personal acquaintance of the men with whom he corresponded before he came to Paris, and especially Antoine Arnauld and de Carcavy. The latter belonged to the circle in which Pascal moved Whether at that time Leibniz had made the acquaintance of Huygens is not quite so certain; at any rate he did not come into close relations with him until after his return from London. Huygens presented him with a copy of his great work, *Horologium Oscillatorium,* which had just (1673) been published. The recognition that his mathematical knowledge at that time was insufficient to enable him to understand the contents of this book, combined with a reawakening of his former love for mathematics, had the effect of making Leibniz devote himself with the greatest fervor to the study of mathematcial subjects. Cavalieri's method of indivisible magnitudes, the writings of Gregory St. Vincent, the letters of Pascal (which were especially recommended to him by Huygens), were used by him as guides in his studies. As the first-fruits of these studies, he obtained the theorem that, when the square on the diameter of a circle was taken as unity, the area of the circle was expressed by the infinite series

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[8] The account given by Leibniz himself in the *Historia* (see *The Monist* for October, 1916) reads thus: "He" [for Leibniz wrote in the third person, under the guise of "a friend who knew all about the matter"] "also came across Pell accidentally, and described to him certain of his own observations on numbers, and the latter stated that they were not new, but it had been recently made known by Nikolaus Mercator.... This made Leibniz get the work of Nikolaus Mercator." As a matter of fact the suggested plagiarism, or what Leibniz took for such a suggestion, was from Mouton and not from Mercator. This is an instance of the lack of memory from which Leibniz suffered; such lack as caused him to make notes of all important points.

[9] See Note 32, on the introduction of the Gregorian calendar.

[10] I cannot see what reason Gerhardt has for this statement, considering the contents of Barrow's book, which we know that Leibniz had purchased; that is, unless we assume either that Leibniz, as I have suggested, did not at that time read the whole of Barrow, or failed to grasp what Barrow had given owing to his (Leibniz's) incomplete knowledge of geometry.

$$1-\frac{1}{3}+\frac{1}{5}-\frac{1}{7}+\ldots\ldots\ldots \text{ ad inf.}$$ [11]

He obtained it thus: Instead of dividing the circle, as in the method of Cavalieri, into trapezia by means of parallels, he divided it into triangles by lines radiating from a point; the areas of these triangles being proportional to certain lines. With these lines as perpendicular ordinates a curve could be constructed that was divided by these ordinates into trapezia, each of which is double the corresponding triangle. In this way Leibniz obtained a curvilinear figure[12] whose area was double that of the circle, but which was expressed by a rational function, $x = y^2/(1 + y^2)$,[13] of its coordinates; and, using a method that was similar to that employed by Mercator for the equilateral hyperbola, this area could be found (Quadratrix).[14]

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[11] Leibniz's own date for the discovery of this result, usually alluded to by him as the "Arithmetical Tetragonism," is 1674; "But in the year 1674 (so much it is possible to state definitely) he came upon the well-known Arithmetical Tetragonism;...." (See *Historia,* in *The Monist,* Oct., 1916.

[12] See the first critical note, page 536.

[13] See the first critical note, page 536.

For the rest of Leibniz's treatment, see the hitherto unpublished manuscript, given under II in the appendix that follows.

As was often the case in the first scientific studies of Leibniz, intimations of the great problems that occupied his attention his whole life through are found here in his first efforts in the domain of higher mathematics. First is it to be remarked that Leibniz abandoned the division of curvilinear figures into trapezia, as was done by Cavalieri, and instead divided them into triangles; from this he was led to the "characteristic triangle,"[15] which formed the foundation in the application of the differential calculus. Further, Leibniz constructed, instead of the proposed curve, another of which the area could be found (the "quadratrix" as he called it); this method of procedure frequently occurs in the later works of Leibniz on the integral calculus. Closely connected also with this is the solution of the inverse method of tangents, that is, given the tangent, to find the curve.

In these first efforts of Leibniz in the domain of higher mathematics is clearly to be seen the influence of his study of the writings of Pascal.[16] The French mathematicians Roberval and Pascal did not consider that Cavalieri's method was consistent with the rigorous requirements of mathematics;[17] they reverted to the study of the Greek mathematicians, and especially to the writings of Archimedes, combining with their method the developments which Kepler, in particular, had brought about by the introduction of infinitely small magnitudes into geometry. Moreover, in connection with Pascal, it is to be observed that he generalized into a "barycentric calculus" the procedure used by Archimedes for the quadrature of the parabola by means of the equilibrium of the lever.[18] This "calculus" enabled him to solve problems on the cycloid which his contemporaries had vainly attempted.[19] It was not unknown to Leibniz that, since the time of Pappus of Alexandria, quadratures and cubatures had been calculated by the aid of the center of gravity (Guldin's rule, "Centrobaryca"); certainly he was now led, by the works of Pascal, again to notice the methods for the determination of the center of gravity, and was also induced to attempt to extend and perfect them. The manuscript of

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[14] Observe that Leibniz (or Gerhardt) employs this word in a different sense from that of Barrow, with whom it means the special curve whose equation is $y = (r - x) \tan \pi x/2r$, a curve that is particularly connected with the circle.

[15] This contradicts both Gerhardt and Leibniz himself, who said that he got it from a consideration of a figure used by Pascal in finding the content of the sphere. See also the critical note referred to in 12, 13 above.

[16] I hope to consider this influence in a later number of *The Monist*, in connection with an essay by Gerhardt on this very point; when I shall endeavor to substantiate an opinion I have formed with regard to the earlier manuscripts of Leibniz, which were discovered by Gerhardt, and of which translations appear in *The Monist* (April, 1917). I suggest that these do not represent so much the record of his original investigations as notes made while using the works of his predecessors as text-books.

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[17] I fail to see how this statement can be completely reconciled with the following well-known quotation from the "*Lettre de A. Dettonville à Carcavy*" (1658):

"*J'ay voulu faire cét advertissement pour monstrer que tout ce qui est demonstré par les veritables regles des indivisibles se demonstrera aussi à la rigueur et à la maniere des anciens; et qu'ainsi l'une de ces Methodes ne differe de l'autre qu'en la maniere de parler; ce qui ne peut blesser les personnes raissonnables quand on les a une fois avertyes de ce qu'on entend par là*" (Vol. VIII, p. 352).

Pascal also says on p. 350: ".... *la doctrine des indivisibles, laquelle ne peut estre rejettée par ceux qui pretendent avoir rang entre les Geometres.*"

That is, the method of indivisibles does not differ from the method of exhaustions, except in the way the argument is put; and that the former must be accepted by any mathematician with pretensions to rank among geometers.

The page reference is to the edition of Pascal's Works in 14 volumes, in the series, *Les Grands Ecrivains de la France* (pub. Hachette et Cie., Paris, 1914).

[18] Pascal calls it "la balance." It is worth noting in this connection that Pascal uses the word "*force*" and not "*moment*" for the product of one of his weights and its lever-arm; so that we must look elsewhere for the clue to the use of the word "moment" in this sense by Leibniz.

[19] Several of the problems proposed were solved by Huygens, de Sluze, and Wren; but by special methods, which did not satisfy Pascal, who called for a general method. Later (1670) Barrow gives the rectification of the arc, as a special case of a general theorem (Lect. XII, App. 3, Ex. 2, see my *Barrow*, p. 177).

Leibniz which is dated October 25, October 26, October 29, November 1, 1675, and which contains the investigation on the center of gravity, is headed, "*Analysis Tetragonistica ex Centrobarycis.*"[20]

It is worth remarking that in this Leibniz continues the method by which he had found the series for the area of the circle. Incidentally these studies were the first occasion for the introduction of the symbol for a sum, i. e., the integral sign (October 29, 1675); from this as the antithesis, the sign for the difference, i. e., the symbol for differentiation, resulted.[21] The equation in which Leibniz first introduced the sign of integration was, in the notation of that time:

$$\frac{\overline{\text{omn.}\ l}\,\boxed{2}}{2} \sqcap \overline{\text{omn.}\ \overline{\text{omn.}\ \frac{l}{a}}}$$

that is,

$$\frac{(\text{omn.}\,l)^2}{2} = \overline{\text{omn.}\ \overline{\text{omn.}\ \frac{l}{a}}}\,;$$

for which Leibniz writes

$$\int\frac{\overline{l}^2}{2} \sqcap \int\overline{\int\overline{l}.\frac{l}{a}}$$

that is, when $l = dy$,

$$\frac{y^2}{2} = \frac{1}{a}\int dy \int dy$$

After his return to Paris in March, 1673, Leibniz was in constant communication with Oldenburg, the Secretary of the Royal Society; the subjects being almost entirely mathematical. In this way he obtained his knowledge of the work of the English mathematicians. Oldenburg's mentor on all mathematical questions was John Collins, who possessed a very wide acquaintance among English mathematicians; and it was through him that what they had done was communicated. In this respect special mention is to be made of the letter from Oldenburg to Leibniz, dated July 26, 1676, in which Collins informed him of a collection of letters from English mathematicians that he had in his possession. Collins mentions in it particularly that script of Newton, of December 10, 1672, in which the latter makes a communication about his method for tangents to curves, which are given by an explicit algebraical equation; he remarks that the method is only a corollary to a general procedure for solving other problems, such as those relating to rectification, determination of centers of gravity and so on.[22] Collins stated in addition that, besides what this letter showed, nothing further was known at that time about Newton's method. It was on account of these communications, and probably also on account of a letter from Newton to Oldenburg, of which Oldenburg sent a copy to Leibniz at Paris, that Leibniz was moved to make his return journey to Germany in October, 1676, by way of London. Leibniz stayed there about a week; he made the acquaintance of Collins, who willingly let him have access to his collection of treatises and letters.[23] What Leibniz found in them that he thought worth noting he set down on two folios; the one has the heading, "*Excerpta ex tractatu Newtoni de Analysi per aequationes numero terminorum infinitas.*" This is the paper which Newton sent in June, 1699, to Barrow, from whom Collins received it on July 30, 1699. Collins made a copy of it, and sent the original back; and the original was printed in the year 1711. The other sheet has the heading, "*Excerpta ex Commercio Epistolico inter Collinium at Gregorium.*" A partial transcript of both these sheets follows under the heading III.

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[20] A translation is given in *The Monist*, April, 1917.

[21] See the second critical note, page 543.

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[22] Leibniz, in the *Acta Eruditorum* for the year 1700, says, "I can affirm that, when in 1684 I published the elements of my Calculus, I did not know any thing more of Mr. Newton's inventions in this kind, than what he formerly signified to me by his letters, viz., that he could find tangents without taking away surds;...." As Newton says in the article in *Phil. Trans.*, Vol. XXIX, No. 342, Anno 1714 (usually called the "*Recensio*") this "is very extraordinary, and wants an explanation."

[23] This is feasible, but there is another alternative given by Dr. H. Sloman (*The Claim of Leibniz to the Invention of the Differential Calculus, English edition,* pub. Macmillan, 1860), which strikes me as even more probable. Sloman's points are as follows: (1) It is highly probable that Leibniz's week in London was the *last week of that month.* (2) Oldenburg had then in his possession two letters from Newton for Leibniz, dated Oct. 24 and 26; these he showed to Leibniz. (3) As Newton himself mentions, these were blotted and hastily written; and thus Leibniz asks, on this account, that Oldenburg should let him see the tract of Newton to which they refer; which tract Leibniz knew was in the possession of Oldenburg, that is, a copy of it. For the details of the argument, occupying ten quarto pages, see the above-mentioned book by Sloman, pp. 97-106.

With regard to the extracts from Newton's paper, it is to be remarked that Leibniz was interested in the treatment of algebraical expressions of powers and in the turning of irrational expressions into the form of series by means of division and root-extraction. He noted indeed many examples in their entirety. How to get to quadratures was known to him; he merely indicated the process by the sign of a sum, i. e., by the symbol of integration. On the other hand, the part on the numerical solution of adfected equations was new to him, and this he copied out well-nigh word for word; this is the well-known Newtonian method of solution of equations by approximations. Leibniz passes over as well known to him the remark, made by Newton at the close of the quadratures, that the problems of rectification, determination of the content of solids, determination of the centers of gravity, can be solved in the same way, and also the general indication of the process to be followed in such cases. Then follows the solution of inverse problems, for instance, to find from the area the base, that is the axis of the curve. This Leibniz copied out word for word. In the same way Leibniz has extracted the conclusion of Newton's paper, "*Demonstratio resolutionis aequationum affectarum.*" At the end of his manuscript Leibniz adds: "I extracted this from the letter of

Newton, August 20, 1672, addressed to Newton."[24] Probably this means that from the letters referring to Newton, Leibniz picked out the letter dated August 20, 1672, addressed to Newton.[25] So far as the script can be deciphered,[26] its contents were a graphic representation of Newton's method of solution of equations by approximations by means of Gunter's scale. Gunter's line had been noted by Leibniz on his first visit to London.

Of quite special interest to Leibniz were the letters of mathematicians which Collins had collected; on a second folio he made excerpts from letters from James Gregory. In two letters from Gregory (1670) was Isaac Barrow extolled as the greatest, not only among living writers, but also among all those that had written before him (Barrow). Further Leibniz found among these letters the letter mentioned above of Newton to Collins of December 10, 1672;[27] he extracted what Newton had mentioned with regard to his method of finding the expression for the tangent to a curve. Leibniz added at the end of this extract, "This method differs from that of Hudde as well as from that of Sluse, in that irrationals need not be eliminated."[28]

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[24] The Latin, "*Excerpsi ex Epist. Neutoni 20 Aug. 1672 ad Neuton,*" as given by Gerhardt, seems somewhat unintelligible; especially the word *Neuton.* What Collins had (or what Oldenburg, as suggested by Sloman, had) was a copy of a manuscript that Newton had sent to Barrow. Gerhardt says, "so far as the script can be deciphered"; perhaps the word *Neuton* is an error of transcription, or maybe an error on the part of Liebniz, due to the juxtaposition of the *Neutoni* which comes just before. In any case, note 25 applies.

[25] I do not think Gerhardt's translation of the word *excerpsi* is correct.

[26] Gerhardt does not state whether the extract is badly written (this would show that it had been done in a very great hurry, for Sloman says that Leibniz, in his matter for publication, wrote a beautiful hand), or whether spoilt by age; in the latter case, as old-time inks contained salts of iron, the manuscript might be restored by photography, by means of a special plate, that I understand is sometimes used for detecting forgeries in deeds and notes.

[27] The letter was sent to Barrow to be sent on to Collins, probably with the object of being communicated through the latter to others; Collins seems to have been the regular channel of communication at this period, in a similar way to Mersenne.

[28] So we find in a manuscript, dated July 11, 1677, first of all an allusion to Sluse's method of tangents, "in which the equation is purged of irrational or fractional quantities"; then the remark, "I have no doubt that the gentlemen

From these extracts it follows that the contents of Newton's letter were unknown to him at that time (Oct., 1676).[29]

Regarding the verbal communications that Leibniz had from Collins during the second stay in London, Collins wrote to Newton from London on March 5, 1677 (1676 O. S.), that the representation of the roots of an equation by a series was discussed between them.

It is clear that Leibniz during his second stay in London had made himself more familiar with the results obtained by English mathematicians than he was before. The question now arises: What specially occupied his attention? What had particular influence upon his studies? It is seen that what Leibniz found in Collins's collection relating to algebraical analysis was new to him and excited his interest; also the verbal exchange of ideas between himself and Collins was upon the same subjects.

On the other hand, as regards the infinitesimal calculus, Leibniz obtained nothing during his second visit to London; he had made a progress, by the introduction of his algorithm into the higher analysis, beyond anything that came to his knowledge in London.[30] Also these algebraical results, at least for the next period, left behind no lasting impression; for among Leibniz's papers is to be found an extensive treatise, written on board the ship that carried him from London to Holland, wherein he considered the

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I have just mentioned know the remedy that is necessary to apply"; then follows the rule for a quotient, and the remark that this will be sufficient for fractions; lastly the rule for powers, with the remark that this will be sufficient for irrationals. Later, he says, "This method has more advantage over all others that have been published than that of Slusius over all the rest, because it is one thing to give a simple abridgment of the calculation, and quite another thing to get rid of reductions and depressions."

Thus, after the sight of Newton's paper, his whole business has been to improve the method of Sluse.

[29] I read it quite otherwise; he has had information of some kind, whether from Oldenburg direct or from Tschirnhaus, while in Paris, and visits London with the express intent of seeing the original papers.

[30] See the third critical note, page 546.

fundamental principles of motion, in the form of a dialogue.[31]

It was in the letter to Oldenburg written from Amsterdam on November 18/28, 1676,[32] which Collins spoke of in the letter to Newton mentioned above, that Leibniz first refers to the subject of the problem of tangents, and remarked that the method of Slusius was not yet very perfect.[33]

KARL IMMANUEL GERHARDT.

CRITICAL NOTES ON GERHARDT'S ESSAY.

BY THE TRANSLATOR.

NOTE I. *The origin of Leibniz's "transmutation of figures."*

(Referred to in footnotes 12, 13, 15.)

In the manuscript, which follows under heading II, Leibniz appears to attach very considerable importance to the method of transmutation of figures, and to claim that he had originated it. This claim is not incontestible; indeed I am almost inclined to think it is a deliberate plagiarism to start with; but Leibniz has perceived in it something which the original author did not. Can it by any chance be the case that, in conformity with several other instances of Leibniz's bad memory for details, he is confusing author and subject, when he speaks of "the great light that suddenly dawned on him, which the author had missed," the reference being to Pascal and the discovery of the differential triangle? Can it be that the true connection is that in considering the original work of the author of such transmutations of figures, he perceived the method for the arithmetical quadrature? For here he really has found a thing that the author missed though it was almost staring him in the face, his discovery being due to a habit that Leibniz had of writing down everything that he could get out of any particular figure or bit of work that he had in hand, whether it was relevant or irrelevant.

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[31] Could this possibly have had its rise in an effort on the part of Leibniz to understand fluxions, or rather the idea of fluxions as he had found it in Newton's paper?

[32] In 1582, Gregory XIII had directed 10 days to be suppressed from the calendar, then in accordance with the Julian system of intercalation, in order to allow the error which had crept into the time of the vernal equinox, by which Easter-day was settled, to be put right. The Gregorian calendar was introduced into all Catholic countries the same year, in Scotland in 1600, in the protestant states of Germany in 1700, but not in England until 1752. At the same time the commencement of the legal year in England was altered from May 25 to January 1; thus we frequently find two years given for dates between January 1 and May 25; while there are two days of the month given for all months of the year. For instance, February 1673 in the new Gregorian calendar would be only February 1672 in the Julian, distinguished by the letters O. S. (Old Style); and this date was written February $167^2/_3$. Similarly the date November $^{18}/_{28}$, 1676, was the 28th of November in the New Style, and the 18th in the Old Style, the number of the year being the same, since the day did not lie between the 1st of January and the 25th of May.

[33] "*Methodus Tangentium a Slusio publicata nondum rei fastigium tenet.*" These are Leibniz's words; Gerhardt omits to translate the word *publicata*, which probably refers to the publication in the *Phil. Trans.* of 1672, by Slusius, of the rules of his method, illustrated by examples. Sluse had probably improved upon this before 1676, but there is no evidence on this point. It would seem as if the subsequent work by Leibniz, culminating in the manuscript of July 11, 1677, was largely an attempt to perfect the rule of Sluse *as a rule*, and that Leibniz, if ever, did not appreciate the idea fundamental in the calculus, namely that of rates, until very much later.

Wallis and Pascal had both hinted at the method, i. e., had used it in special cases, namely for proving the equivalence of the parabola and the spiral; and Leibniz was familiar with both these authors. Again, James Gregory had, in the words of Barrow (*Lect. Geom.*, Lect. XII, App. 3, foreword to Prob. IX), "set on foot a beautiful investigation about involute and evolute figures," i. e., polar and rectangular figures equal in area to one another. Of course, Leibniz may not have seen this work of Gregory until later; probably not, although in one of his manuscripts he gives a theorem of Gregory; this however does not count for much, for the very same theorem is given by Barrow (see my *Barrow*, p. 130) and we know that Leibniz had a Barrow in his possession. This book, judging by his words, "as in Barrow, *when his Lectures appeared*, in which I found the greater part of my theorems anticipated," Leibniz wishes to make his friends believe was the 1674 edition, and not the edition of 1670, which he bought on his first visit to London. Why did Leibniz wish to conceal this fact? I assert that the reason for doing so was the fear that seemed always to overshadow him, the fear of being accused of plagiarism, whether such was a true or a false charge. I am firmly convinced that Leibniz got his transmutation of figures from Barrow; to this conclusion I have only just come, it never having entered my head to look for it at the time that I wrote my articles for *The Monist* of October, 1916, April and July, 1917.

Before I bring forward my arguments, it is right to state as a preliminary that, just as in calculus nowadays we usually draw a curve with its convexity downward, and draw the tangent to meet the horizontal axis beneath the curve, so Barrow drew his curves with the concavity downward in many cases, mostly, I think, in order to fit the diagrams conveniently on the old-fashioned folding plates of diagrams, that in those days were added in batches at the end of a book (see a specimen I have given at the end of my *Barrow*); in other cases, he draws his figure on the left-hand side of the axis. Whichever figure he draws, he always did one thing, namely, he drew any supplementary figure he had need of *on the other side of his axis or base.* Leibniz almost invariably drew his curve on the right-hand side of a vertical axis, and supplementary figures on *the same side*. Hence, in the extract from Barrow given below, I am to be excused for failing to notice before what is more than a mere similarity.

In the following extract from Barrow (Lect. XI, Prop. 24), I have added Barrow's proof, which I thought unnecessary to give in my book; the figures given are Barrow's own on the left, which has been "up-ended" on the right; the latter is to be compared with the several figures by Leibniz.

Barrow's Lectiones Geometricae, Lect. XI, Prob. 24.

If DOK is any curve, D a given point on it, and DK any chord; also if DZI is a curve such that when any point M is taken in the curve DOK, DM is joined, DS is drawn perpendicular to DM, MS is the tangent to the curve, DP is taken along DK equal to DM, and PZ is drawn perpendicular to DK, so that PZ is equal to DS; in this case the space DZI is equal to twice the space DKOD.

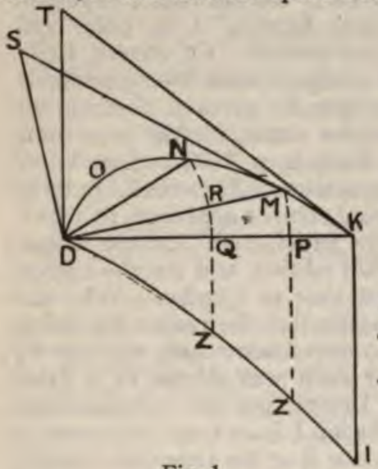


Fig. 1.

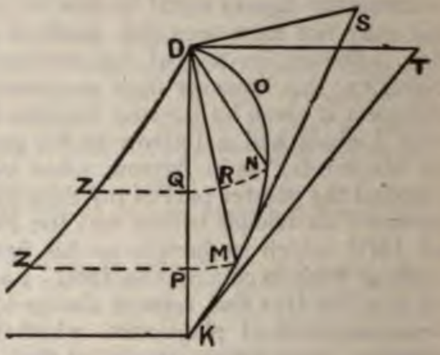


Fig. 2.

For let KP be considered to be indefinitely small, and let DT be perpendicular to DK and KT the tangent to the curve DOK. Then, drawing the arc MP, we have *as before,*

$KP : PM = KD : DT = KD : KI$, and hence $KP . KI = PM . KD$.
Take another small part PQ and, with center D, draw an arc QN through Q cutting the chord DM in R; then *as before,*

$MR : RN = MD : DS$, $PQ : RN = MD : PZ$, $PQ . PZ = RN . MD$;
and so on one after the other. Therefore, it is evident that the sum of all the rectangles KP.KI, PQ.PZ, etc., is equal to the aggregate of all the spaces PM.KD, RN.MD, etc.; that is, the space DKI = 2 times the space DKOD.

The words I have italicized refer to Prop. 22, in which he uses a similar though rather more complicated figure to reduce a polar area to a rectangle *of which one side is a given straight line,* and explains that the reasoning depends on the fact that the line DK is divided into infinitely small parts. Compare the words I have italicized with the description of Leibniz's method: "the areas of these triangles *being proportional to lines.*

Further, Barrow proceeds in Prop. 25 to prove the equivalence of the spaces formed (i) by applying each MS to the base and (ii) by applying each chord to the arc, previously rectified. And he winds up with the words: "Should any one explore and investigate this mine, he will find very many things of this kind. Let him do so who must, or if it pleases him."

This all suggests that Leibniz *did* explore this mine, that he did *not* invent the method of transmutation of figures for himself,

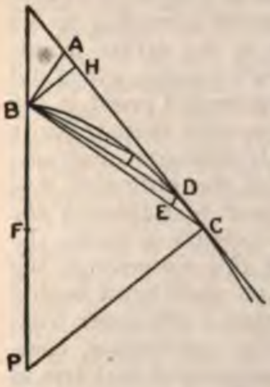


Fig. 3.

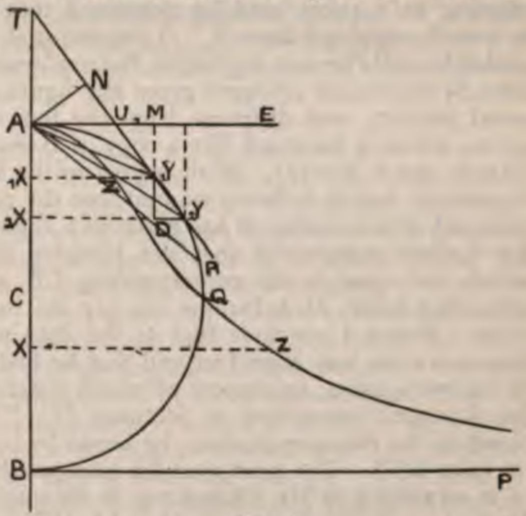


Fig. 4.

that he *did* find very many things of this kind, and that Barrow had *missed the arithmetical quadrature* construction; this Leibniz obtained through his regular practice of working every mine right out, to keep up Barrow's simile. Further comment is needless, I think, after a comparison of Barrow's figure (the up-ended version) with the figures of Leibniz given above.

Fig. 3 occurs in a manuscript November 21, 1675, which according to Leibniz is at least a year after he had discovered the arithmetical quadrature; and yet it has a heading, "A new kind of Trigonometry of indivisibles, etc." In this figure it is to be noticed that he has the perpendicular to the chord BC, agreeing with Barrow's DS and DT, but has not the tangent at the vertex that was necessary for the demonstration of the arithmetical quadrature. In the working in connection, he considers the similarity of all the triangles possible, and notes as one point that "*the sum of all the triangles or the area of the figure is equal to the products of the AB's into the CE's,* which is Barrow's proof of Prop. 24 above.

Fig. 4 is the figure given in the *Historia* (see *Monist* for Oct., 1916), in connection with the explanation of how he found the area of the circle. Notice the difference between this figure and the one given in the manuscript that follows under the heading II, also that the description there given of the way in which he was led to it is much more natural. This is probably the true version, for the use of the notation B, (B), ((B)), points out that it was written at a comparatively early period, before Leibniz had adopted the prefix notation, ${}_1B$, ${}_2B$, ${}_3B$. In the account in the *Historia,* to which Fig. 4 applies, Leibniz says, "he once happened to have occasion to break up an area into triangles formed by a number of straight lines meeting at a point, and he perceived that something new could be readily obtained from it." I suggest that the occasion was most probably while he was digging in Barrow's mine! This is the reason why he has in the *Historia* given the figure more according to his usual practice, and different from the figure in the earlier manuscript, which is too much like a copy of Barrow's (query, where did Barrow get it from?). With regard to the figure and proof in the manuscript which follows, we find that the reasoning there given is unsound, unless Gerhardt has given us a slightly erroneous diagram; for Leibniz apparently does not perceive that the ordinates BA, which are equal to the corresponding CE, *must pass through the respective points D,* before he can say that one figure is double the other. Hence I conclude that at the date of this manuscript, the demonstration was imperfect and that he had no proof until he dug in Barrow's mine; in support of which conclusion I will quote from the *Recensio,* mentioned in footnote 22. "This quadrature, composed in the common manner, he began to communicate at Paris in the year 1675. The next year he was polishing the demonstration of it, to send it to Mr. Oldenburg, in recompense for Mr. Newton's Method, as he wrote to him May 12, 1676; and accordingly in his letter of August 27, 1676, he sent it, composed and polished in the common manner." This polishing, I take it, consisted in making the slight but important alterations in the demonstration and figure, from those given in the manuscript II that follows, to those given in the *Historia.*

What had he then got in July 1674, when he wrote to Oldenburg saying that he had got a wonderful Theorem, which gave the area of a circle, or any *sector of it exactly,* in a series of rational numbers? Or, when in the October following, October 26, 1674, he wrote to say that he had found the circumference of a circle in a series of very simple numbers; and also by the same "method" (a favorite expression of Leibniz) *any arc whose sine was given?* It was impossible that Leibniz could have had the two things that I have italicized; or at least, the latter was impossible to him, because the only way for him to obtain it *exactly*, i. e., to know the law of his series, was as yet unknown to him; unless we are to assume, contrary to his assertion, that the binomial theorem was known to him, which would involve his also having seen or been told about other parts of Newton's work. The only way open to Leibniz was to find the square root of $1-x^2$, and then its reciprocal by division; and this would not give him the law of the series, even if we assume that his knowledge of integration was sufficient to enable him to proceed any further. From his manuscripts it does not seem that even up to Nov. 1675 he had any further knowledge of integrations than that $\text{omn.}x=x^2/2$, and $\text{omn.}x^2=x^3/3$; but as he says that he knows the latter from the quadrature of the parabola, there is some possibility that he might have been able to integrate every integral power of the variable from his reading of Wallis and Mercator.

However, there is the strongest probability that he had not got any proof for the two things italicized, and that the quadrature was in the same category. Where then had he obtained it? We find that in December, 1670, Gregory had found out for himself Newton's method of series; and two months later, February 15, 1671, sent several theorems to Collins, one of which was that now known as "Gregory's series." "And Mr. Collins was very free in communicating what he had received both from Mr. Newton and Mr. Gregory, as appears by his letters printed in the Commercium" (from the *Recensio*). One can imagine that Oldenburg would be one of the first to receive the information, and that for a certainty it would be passed on to Leibniz. I think then that Leibniz perceived that by putting $x=1$ in Gregory's series, and making the radius of the circle equal to unity, he could get an arithmetical quadrature; from that time onward he looked for a proof by pure geometry, and found it after reading Barrow's proposition referred to above; if we assume the possibility of integration of integral powers, it was an easy step to find that the series he had to integrate was $y^2/(1+y^2)$, and all he had to look for on his figure was a line of this length. This very well accords with the description of the way in which he found his demonstration, as given in the manuscript which follows under the heading II.

Lastly, in connection with the suggestion that I have made above, namely, that Leibniz had another method for his arithmetical quadrature than those he has given, there is one method that is bound up with the change that he made from the Pascalian characteristic triangle which he used at first, to the Barrovian differential triangle (see my note in *The Monist,* Oct., 1916, p. 615). In Example 5 of the method of the differential triangle (see my *Barrow,* p. 123), Barrow has found the subtangent for the curve $y=\tan x$, from a consideration of the figures on next page, and finds that

$$t=\frac{rr}{rr+mm}m.=\frac{\text{CB}^2}{\text{CG}^2}.\text{BG}=\frac{\text{CK}^2}{\text{CE}^2}.\text{BG}\,.$$

where r is the radius of the circle, m is the ordinate MP, which is equal to BG, and t is the subtangent TP.

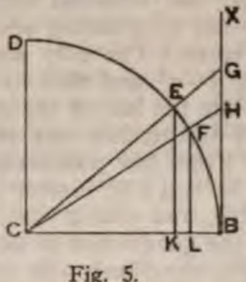


Fig. 5.

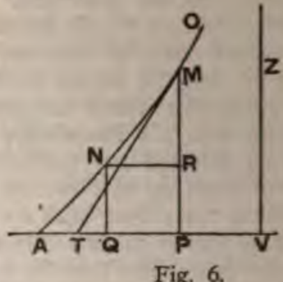


Fig. 6.

Now if we put the radius equal to unity, and for the ratio t/m substitute what was known by Leibniz to be equal to it, namely, QP/RM or EF/GH (by construction), we have the sum of all the EF's is equal to the sum of ordinates equal to CK^2 (radius = 1) applied to G at right angles to BG. Analytically, calling BG z, we have

$$\text{arc BE} = \text{sum.omn.}\frac{1}{1+z^2} \text{ applied to the line } z;$$

hence by division

$$\begin{aligned}\text{arc BE} &= \text{sum.omn.}(1 - z^2 + z^4 - z^6 + \text{etc.})\\ &= z - z^3/3 + \text{etc.}\end{aligned}$$

I can hardly see how Leibniz could have missed this with his analytical mind, even although Barrow has missed it; but there is a strong probability that at the time of writing, Barrow had not seen the quadrature of the hyperbola by Mercator, and, if he had, such algebraical work would not have appealed to him at all.

As far as I can make out, there is only one other alternative, which involves a direct contradiction of Leibniz's own statement; that is that his proof was not by the transmutation of figures in the first instance. Color is lent to this view by a letter of Leibniz and other papers, quoted by Sloman (pp. 131ff, in the English edition of the work referred to in footnote 23); also even by a passage in the *Historia* (see *Monist,* Oct., 1916, p. 599), where, while giving the story of the discovery of the arithmetical tetragonism, Leibniz distinctly hints at an algebraical method; for he says immediately afterwards, "The author obtained *the same result by the method of transmutations,* of which he sent an account to England." This reads as if he had another method in addition to the method by transmutations.

Let us consider this algebraical method. To square the circle, Leibniz has to integrate $\sqrt{(1-x^2)} = y$, say; let $y = 1 - xz$, then $y = (1-z^2)/(1+z^2)$, which is rational; moreover, he would also have been able to have substantiated his statement that at this time he also had a proof of the series for the arc whose sine was given, for which he would only have had to integrate $1/\sqrt{(1-x^2)}$. But one cannot conceive that Leibniz had any means of expressing the element of z in terms of the element of x. Geometrically, he was incapable of it, without using Barrow's infinitesimal method; and of this we find the first instance in a manuscript dated November 1, 1675. Algebraically, he could not, for at this same date he could not differentiate a product. How then are we to account for the fact that he says he has a method for demonstrating both series for the arc, given the sine or the tangent? I think I can answer this. Many times we find assertions made, not only by Leibniz in those times, but by others in other times, of the possession of discoveries, when all that the assertor has is the idea of how they may be obtained. Thus, in the passage quoted, the concluding statement is, "and thus again all that remains to be done is the summation of rationals." So that if we accept this alternative we are bound to come to the conclusion that Leibniz did not yet recognize, what he ought to have done from the work of Pascal, that an area was not a mere summation of lines, but of rectangles formed by these lines ordinated at certain definite points along a straight line. That is to say, he did not recognize the fundamental principle of integration, namely, the importance of the factor dx or dz. When he had to write out his proof he found that the summation of $(1-z^2)/(1+z^2)$ or its reciprocal was beyond him; or rather that the series he found by Mercator's method was not correct; he had to resort to the geometrical proof, of which he got the idea by digging in Barrow's mine, as above; he found that this would not work for the other series; and consequently he dropped all claim to the second series. In his letters of 1676, therefore, we find him offering to send Newton the proof of his quadrature in return for the method of proof of the series for the arc when the sine is given.

Thus I come to the conclusion that Leibniz obtained these series in some way by correspondence, thought he had got a proof of his own, (which turned out to be incorrect), and much later did obtain a proof of his arithmetical quadrature by the transmutation of figures, *after obtaining the idea from Barrow.* As the special case, when x = the radius, had not been specifically mentioned by Gregory, Leibniz considered that he had a right to claim it, more particularly as he thought he had devised a proof for it, if it was necessary to produce one; for of course, Gregory had given no proof according to the usual custom of the time. Then, when he did find a proof, after having found that his original idea was hopeless, one can hardly blame him for sticking to his claim.

NOTE 2. *On the introduction of the Leibnizian algorithm.*

(Referred to in footnote 21.)

The two passages in which the signs for integration and differentiation are respectively introduced occur in the manuscript of October 26, 1675.

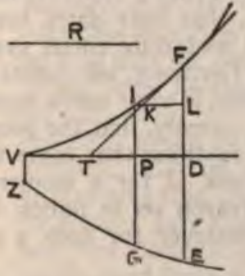
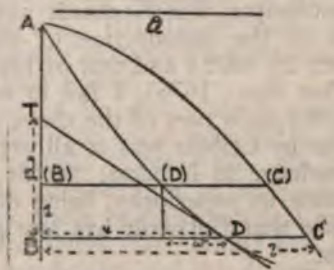
i. "It will be useful to write $\int$ for omn., so that $\int l = \text{omn. } l$, or the sum of the l's."

ii. Not for some time is the sign for differentiation introduced, and then in these words: "I propose to return to former considerations. Given l and its relation to x, to find $\int l$. Now this comes from the contrary calculus, that is to say if $\int l = ya$. Let us assume that $l = ya/d$, or as $\int$ increases, so d will diminish the dimensions. But $\int$ means a sum, and d a difference. From the given y, we can always find ya/d or l, or the difference of the y's. Hence one equation may be changed into the other,....."

Now of these the introduction of the symbol for integration can no more be called an invention than the use of Σ to stand for "the sum of all such terms as." It was simply, as Leibniz himself says, a convenient and useful abbreviation for sum.omn. or omn. It is nothing more or less than the long *s* then in general use; indeed it was so thought of by contemporary mathematicians, Newton for one at any rate, for we find in the *Recensio* the passage, "Mr. Leibniz has used the symbols sx, sy, sz for the sums of ordinates ever since the year 1686." This may have been an instance of prejudice, or perhaps the printers of the *Phil. Trans.* may not have had an integral sign in their fonts of type; but it shows up the fact that the English accepted it as the initial letter of the word "summa."

Now let us consider the introduction of the letter d. Gerhardt says that it resulted as antithesis to the sign $\int$. How he can possibly derive this from the context I cannot surmise. I am well aware that in another passage he was unable to assign a meaning to the introduction of a letter, which was, to me, clearly used for the simple purpose of keeping the dimensions correct. We have this use again in the present passage. Leibniz knows that the sum of the lengths, $\int l$, is an area; hence taking y to represent a length, given in terms of x, he introduces the *length* denoted by a to give with y the area of a rectangle. Therefore he argues that l must be an area divided by a length, and he writes $l = ya/d$, where d is *another length, introduced to keep the dimensions correct.* This is clear from the sentence that follows next: "so will d diminish the dimensions."

So far the sequence of ideas is easy to follow, and there is not the slightest trace of any concept of differentiation, nor, if the l's are ordinated to any axis, any trace of a connection between d and an element of that axis. The difficulty begins with the next sentence: "But $\int$ means a sum, and d a difference." The first idea that strikes one is that this was added later, after that he had found out the connection between the inverse-tangent problem and quadratures. Gerhardt gives no suggestion on the point, so until the paper can be reexamined for small details like differences in the ink or character of the writing this idea will be disregarded. The next is that about this time he was reading Barrow, and then one is at once reminded of Lect. X, Prop. 11; this is the proposition in which Barrow proves that differentiation is the inverse of integration. If we consider this in the manner of Leibniz, we get the equivalent that is set down on the right-hand side below:

BARROW	LEIBNIZ
Let ZGE be any curve of which the axis is VD; and let ordinates applied to this axis, VZ, PG, DE, continually increase from the initial ordinate VZ;	Let AC be a curve, whose axis is AB, and let the ordinate AB be l;
also let VIF be a line such that if any straight line EDF is drawn perpendicular to VD, cutting the curves in the points E, F, and VD in D,	let AD be another curve, having the same axis, and let its ordinate DB be called y;
the rectangle contained by DF and a given length R is equal to the intercepted space VDEZ;	let this curve AD be such that the area ABC, i. e., all the l's or $\int l$, is equal to the product of BD and a fixed line, i. e., equal to ay;
also let DE:DF=R:DT, and join DT. Then TF will touch the curve VIF.	then, taking B(B) equal to unity, we have $l=aw$, where w:B(B) =DB:BT, or $w=y/d$, i. e., $l=ay/d$.
Cor. It should be observed that DE.DT=R.DF=area VDEZ.	
	

We thus see that the d that results as the "antithesis to the integral sign" (*als Gegensatz....sich ergab*), is not a difference at all, but the subtangent; it is y/d or w (on account of B(B) being taken as unity) that is the difference between the ordinates y. But there is not the slightest trace of the idea of differentiation; this is made more manifest by the work which follows, which is based on his idea of obtaining independent equations, and eliminating all variables but one and thus reducing the problem to a quadrature. And yet he seems to perceive from the equation that gives the difference of the y's as a *quotient*, that in some unintelligible way a division means a difference. Later therefore we find him trying to find an interpretation of d as an operator, whether he writes it in front of his y, or as a denominator; namely, when he considers what value he is to assign to $d(xy)$. I venture to assert, unless we assume that Leibniz is considering this proposition of Barrow's, that there is no possible connection to be made out between the several sentences of this passage. Also that in no sense can this introduction of the letter d be looked on as an algorithm with any idea in it of differentiation.

I am well aware that in the above I have adduced no positive proof that my idea is correct; I have not had the advantage of Gerhardt in seeing these manuscripts. But I have honestly tried to find other ways of explaining the circumstances that lead from y/d as a *quotient* to dy as a *difference*, and I can find none other that is feasible than that given above, namely, that, perhaps by accident, Leibniz uses d for the subtangent (instead of the usual t), and perceives from such a figure as the above (which of course I do not intend to say he has given) that y/d (where d is the subtangent) works out the same as dy (when dx is taken to be unity); in other words the subtangent d is equal to $y/(dy/dx)$.

NOTE 3. *On the progress made by Leibniz before November, 1676.*

(Referred to in footnote 30.)

The remark made by Gerhardt that Leibniz "had made a progress, *by the introduction of his algorithm into the higher analysis,* beyond anything that came to his knowledge in London," is, to say the least of it, a matter of opinion. From a study of the six manuscripts, that Gerhardt has given us, that bear dates between that of the introduction of the integral and differential symbols (Oct. 26, 1675) and that of his return to Germany, via Amsterdam (after Nov., 1676), I fail to see that there is very much occasion for the main part of the above statement, namely, that the progress made by Leibniz was at all greater than anything that came to his knowledge in London; as for this progress, if for a moment we assume its superiority, being due to the reason set in italics, I fail to see that Gerhardt has any grounds whatever for such a statement.

The six manuscripts in question have been given, translated into English and annotated in *The Monist,* April, 1917; but for convenience I here add a precis of them.

i. Nov. 1, 1675. A continuation of the work on moments about axes; the new symbols do not occur, *omn.* being still used. He has now read Wallis, Gregory and Barrow, in addition to Cavalieri and St. Vincent; he speaks of his theorem of breaking up a figure into triangles as bringing out something new; the whole tone of this manuscript is in the main Pascalian.

ii. Nov. 11, 1675. He successfully obtains a solution of the problem of finding a curve such that the rectangle contained by the subnormal and ordinate is constant. This he considers to be "one of the most difficult things in the whole of geometry." He uses the integral sign, and the denominator d; but neither integration nor differentiation, the fact that $y^2/2d=y$, being taken from the "*quadrature of the triangle.*" In verifying his result he quotes Slusius's Rule of Tangents. Further on, he has the note that x/d and dx are the same thing, though there is nothing to show why he comes to this conclusion; see the last critical note. He also comes to the conclusion that $d(xy)$ is not the same as $dx.dy$; but in the last bit of work in this manuscript he uses special letters for the infinitesimals, showing that he has been trying to find the effect of d as an *operator*, or perhaps trying to find the reason of the equality x/d and dx. He has failed to solve a problem, which results in the differential equation, as we should now write it, $x+y.dy/dx=a^2/y$, or as Leibniz has it $x+w=a^2/y$; although he gives an incorrect solution, which he asserts to be true. This time he does not attempt to verify his solution, the reason being obviously that he is unable to do so, because one side of his equation is a product. As a matter of fact, I have it on the authority of Professor Forsyth that there is no solution of this equation in elementary functions; or at least he says that he has been unable to find one, which I take it comes to the same thing. The one advance that can be found here is the appreciation that squares and products of infinitesimals can be neglected, as he has doubtless found in reading Barrow. It is worth noting that he now uses the differential triangle in Barrow's form instead of the form he says he got from Pascal.

iii. Nov. 21, 1675. In this manuscript he sets himself another problem, which he fails to solve; the curve required is logarithmic, and this fact even he fails to bring out. In generalizations that arise from the consideration of his problem he obtains $dx\,y=xy-x\,dy$, in a more or less analytical manner; but immediately afterward states that nothing new can be obtained from it; he has already obtained this formula by his consideration of moments, geometrically; and he does not appreciate the advance there is in obtaining it algebraically. The manuscript concludes with a consideration of the figure by means of which it is generally supposed that he affected his arithmetical quadrature. This is very remarkable on account of the heading, which reads, "A *new* kind of Trigonometry of indivisibles, by the help of ordinates that are not parallel but converge." What I refer to is the use of the word new, which I have here italicized. It is to be observed that the diagram and the results are almost identical with those of *Barrow*, Lect. XI, Prop. 22-24 (see the first critical note). He concludes by a reference to the trochoids, which shows that he is still under the influence of Pascal, if indeed he is not still studying his works.

iv. Nov. 22, 1675. He returns to the subjects of the previous day. But there is here no mention of the signs of integration or differentiation.

v. June 28, 1676. Here we have a certain advance, for there occurs the statement: "The true general method of tangents is by means of differences." While he uses dy and dz for the elements of y and z, he uses β for the element of x; the rest of the work is merely Barrovian in principle. This mere substitution of dy and dz for the special letters used by Barrow for the same things can hardly be called progress. What progress there might be is barred by the use of equations with three or more variables in them.

vi. July, 1676. The remark on the last manuscript is corroborated by the contents of this manuscript. Leibniz asserts that he has solved two problems, of which Descartes had alone solved one, and owned that he could not solve the other. The truth is that he has not solved the former, which was fairly easy, only given an alternative construction which is, if anything, more difficult to carry out than a construction from the original data for the curve. The latter he gets out in a hazy fashion ("....which belongs to a logarithmic curve"). This conclusion he comes to after several erroneous steps of reasoning; whereas the solution stared him in the face about a quarter of the way through the work, where he has the equation $c\,dy=y\,dx$, if he could have integrated dy/y with certainty. The failure I think arises from the study of Pascal, who lays it down that only one of the variables can increase arithmetically, and Mercator's work has been with y increasing arithmetically, and Leibniz has already considered that the x is increasing arithmetically (See my notes on this manuscript in *The Monist* for July, 1917).

Throughout the whole of these manuscripts, he makes no progress, because he is hampered by the idea of keeping one of his variables increasing uniformly; he seldom uses his algorithm for differentiation; and when he does do so, it is merely a substitution of dx, etc. for the special letters used by Barrow. In fact these manuscripts appear to me to be the records of his work on the text-books of his study, Pascal, Wallis, Gregory, and Barrow; and we see him trying to fit the matter and methods found in them into his own ideas and notations. It is not until November, 1676, when he has arrived on the Continent, after having seen Newton's paper, that we have any Differential Calculus; even then some of the standard forms that he gives are not quite correct; on the other hand, he gives the method of substitution to differentiate an irrational, though he uses the Barrovian method to differentiate the general equation of the second degree, merely using dy and dx instead of

Barrow's special letters. It is not until July, 1677, that he is able to give anything like an intelligible account of the differentiation of products, powers, quotients and roots. Lastly I doubt if Leibniz ever did really appreciate the Newtonian idea that dy/dx was a ***rate***, or else the example he gives of the use of the second and third differentials in his answer to Nieuwentiit would not have contained so many ridiculous errors.

TRANSLATIONS OF THE MANUSCRIPTS

Alluded to by Dr. Gerhardt.

I.

Scientific memoranda of the visit to England at the beginning of the year 1673.

When at the beginning of the year 1673, I accompanied his Excellency the Ambassador of Mainz, Baron Schörnborn, a nephew (on his father's side) of the Elector, from Paris to London, although I stayed in England scarcely a month, among various distractions, I still gave attention to increasing my knowledge of philosophy; for at that time the English held a high reputation in this subject.

To set out a long minute record of daily happenings is useless on account of its inequality; for the fortune of all the days was not the same; indeed the points worth remarking heaped themselves up one day, and the next gaped with emptiness. For this reason perhaps it will be more satisfactory to go by heading of subjects, one remark recalling another as it were.

The principal heads for the subjects noted may be taken as Arithmetic, Geometry, Music, Optics, Astronomy, Mechanics, Botany, Anatomy, Chemistry, Medicine, and Miscellaneous.

ARITHMETIC. The line of proportions or Gunter's lines or the double scale. Logarithmotechnia or compendium for calculating logarithms. To recognize square numbers from non-squares by their end figures. Morland's machine.

ALGEBRA. Substance of English algebraical work of 27 years. Algebra of Pell. At first few rules, but lots of selected examples. Renaldinus not thought much of in England.

GEOMETRY. Tangents to all curves. Development of geometrical figures by the motion of a point in a moving line.

Music. Its universal character. System of Birthincha. Vossius will publish Music.

Optics. They told me of a certain phenomenon that Barrow confessed that he was unable to solve. The difficulty of Newton hitherto unsolved, Father Pardies giving it up. Hook adheres to a catadioptric instrument of 9 feet, because for another of 50 feet movement inconveniences them. The secret of the largest aperture which can be given to microscopes is primarily as great as the distance of the object.

Astronomy. Arrangement of Hook for observing whether the earth at any time sensibly approaches or recedes from the fixed stars, from which it can be judged that it is not in the center of the universe; he erected it in a fine tube set perpendicularly, and observed the stars that are vertically overhead. He, lying flat on his back, observed their dimensions most exactly.

Chemistry.

Mechanics.

Pneumatics.

Meteorology.

Hydrostatics.

Navigation.

Magnetism.

Physics.

Botany.

Anatomy.

Medicine.

Miscellaneous.

II.

[This manuscript is very lengthy, the translation running to about 6000 words, of which the first 5000 are written as a concise history of all the great geometers and their works, that are antecedent to Leibniz himself. This part is quite unimportant for the purpose of estimating the part that was played by Leibniz, and it passes my comprehension why Gerhardt should give it at length, while he has condensed the other two, which are really important. Hence, in what follows, I have given a precis of the first 5000 words, with here and there quotations, in which Leibniz has something to say that is either critical of the work of others, or a claim to superior knowledge or better method of his own. The last part, which purports to be the history of his arithmetical quadrature, together with his claim to the surpassing value of his achievement, I have given in full.]

(Precis). Geometry is a modern thing, probably due to the Greeks. The great name among the Ancients is that of Archimedes, who first used indivisibles; this use was more profound than that of Cavalieri, but the method became lost. The name of Apollonius must not be altogether omitted.

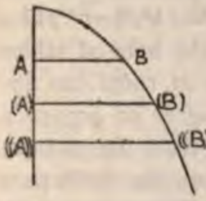
The learning of the Greeks passed on to the Arabs, who conquered them; among these we have Alhazen, and a certain Mahomet, who gave the formula for the general quadratic.

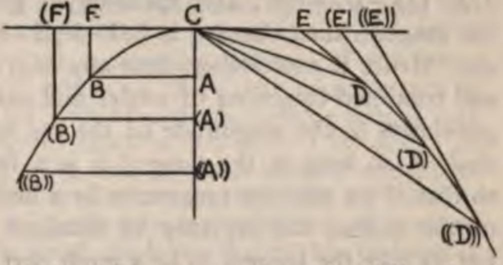
This brings us to the cubic and biquadratic equations, which were solved in the sixteenth century. The cubic is due to one Scipio Ferreus of Bologna; one of his pupils set the solution as a challenge after Scipio's death; Tartalea took up the challenge, found a solution and told his friend Cardan; the latter extended it and published it without the consent of Tartalea. Vieta, Descartes, and Ferrarius gave the solution of the biquadratic. But even Descartes and Vieta failed at equations of higher degrees. With regard to the work of Descartes, Leibniz remarks that "its origin [that is, of the method of solution] was a widely different and more fertile spring; and if Descartes had only recognized this, he would have rendered the discovery of Scipio more general and carried it to further heights. *But what has befallen me in this connection I will say in another place.*" Leibniz further remarks that the method of Descartes fails to give the roots of equations of higher degree, although the quality of the roots may be learned through it. "I will show in another place that the *reason for this is clearly known to me from the most fundamental principles of the art, and that I have established an extremely easy method, and one that is adapted too for enlarging science, by the many things that follow from it.*"

In the seventeenth century, Leibniz goes on to say, after Archimedes and Galileo's several times and influence are gone by, there is no writer from whom more is to be learned than from Descartes; and yet he is "*unable to pass over certain boastful remarks that he makes, by which the less experienced among us may be led into error.*" Descartes had said that by his method every geometrical problem could be reduced to the finding of the roots of equations. Leibniz remarks that this shows Descartes's ignorance of the matter. "For when the magnitude of curved lines or the space enclosed by such is required (which happen more frequently than perhaps Descartes thought, since he had not applied himself sufficiently to the 'mechanics' of Galileo), neither equations nor Cartesian curves can help us, and *there is need of equations of a totally new kind, of constructions and new curves, and finally of a new calculus, given so far by nobody, of which, if nothing else, I can now give certain examples at least, which are remarkable enough.*".... *I have mentioned these things so that men may understand that there are certain methods in Geometry, for which they may look in vain in the works of Descartes.*"

Returning to geometry purely, Leibniz next mentions the work of Galileo, Cavalieri (whose method he considers is rough and limited in extent), Torricelli, Roberval, Pascal, Wallis, Huygens, and Slusius, as contributors to the new geometry. He considers that a new epoch opens with the work of Neil and van Huraet (on rectification of curves), James Gregory, and Brouncker. "Finally Mercator gave a general formula for the area under a hyperbola." He claims Mercator as "an eminent German geometer"; but rather decries his discovery as being an easy one, on account of the ordinates working out as rational in terms of the abscissa. "But it was not so easy to give the magnitude of the circle, and its parts, expressed as an infinite series of rational numbers;.... for the circle, however you treat it, has ordinates that are irrational. However I, as soon as I had found a certain very general theorem, by means of which any figure whatever could be converted into another that is quite different from it, but yet of equivalent area, set to work to try whether the circle could not be converted in some way into a rational figure; and the thing came out beautifully;.... it will be worth while here to give a short account of the matter."

(In full). Nearly everybody who has up to now treated of the geometry of indivisibles has been accustomed to break up their figures into rectangles or parallelograms only by means of ordinates parallel to one another. But the reasoning of Desargues and Pascal always pleased me very much; these in Conics, as we can call them in general, include under the name of ordinates not only parallels, but also straight lines meeting in or converging to a point, especially when parallels are included under the name of converging, by saying that the point of convergence goes off to an infinite distance. Thus while others only consider parallel ordinates, and have broken up their figures into parallelograms AB(B)(A), (A)(B)((B))((A)), in the way that Cavalieri does, I employ converging lines and resolve the given figure into triangles CD(D), C(D)((D)), and at once draw another figure of which the ordinates AB, (A)(B), etc., are proportional to these triangles.





Now this is the case if the AB's are equal to the CE's where it is supposed that the straight lines DE are tangents to the given curve; for in that case, as I will show below, it will come out that the space B(B)(A)A will be double of the segment C(D)DC, and for any figure such as C(D)DC another that is equivalent to it can be drawn. Now, supposing that the curve D(D)((D)) is circular and that CA is a part of the diameter, then, calling CA or FB x, and CF or AB y, and the radius of the circle unity, calculation will show that the value of x is $2y^2/(1+y^2)$. Thus the ordinate FB or x can be expressed rationally in terms of the given abscissa CF or y. Such figures as these, in which the ordinates can be expressed rationally in terms of the abscissae, I call rational. Thus we have drawn a rational figure equivalent to the circle, and this will be soon seen to be sufficient to give the arithmetical quadrature of the latter. For, from the sum of a geometric series of an infinite number of decreasing terms that is well known to all geometers, it follows that $y^2-y^4+y^6-y^8+y^{10}-y^{12}+\text{etc.}$ to infinity is the same as $y^2/(1+y^2)$, i. e., the same as $\frac{1}{2}x$, if only we understand that y is a quantity that is less than the radius, or unity. Now, since we have to collect together the infinite number of $\frac{1}{2}x$'s into one sum, in order to obtain the quadrature of half the figure C(F)(B)BC and what it comes to, namely, that of the circle; so also have we to collect together the infinite number of series $y^2-y^4+y^6-y^8+y^{10}-y^{12}+$etc., into one sum, and this by the method of indivisibles and infinites can be done without difficulty. For, suppose that the last y, which in general is taken as C(F), to be b, then the sum of every y^2 will be $b^3/3$, and of every y^4 will be $b^5/5$, and of every y^6 will be $b^7/7$, and so on; hence, the sum of the infinite number of $\frac{1}{2}x$'s, or of the series $y^2-y^4+y^6-y^8+y^{10}-y^{12}+$ etc., i. e., the area of half the space C(F)(B)BC, will be $b^3/3-b^5/5+b^7/7-b^9/9$ etc. From which, by the help of ordinary geometry, it can be easily deduced that the square on the diameter is to the area of the circle as 1 is to $1/1-1/3+1/5-1/7+$etc.; also speaking in general, supposing b to be the tangent, then the arc is $b/1-b^3/3+b^5/5-b^7/7+b^9/9-b^{11}/11+$ etc. Hence it now follows that any one without the help of tables and continual bisections of angles and extractions of roots can approximate to the magnitude of the arc to any degree of accuracy desired, so long as the tangent b is a little less than the radius; so that if we take the tangent to be a little less than the tenth part of the radius, the arc may be obtained with sufficient accuracy. Let us take the tangent to be a tenth part of the radius, then if we want the arc, it will be

$$\frac{1}{10}-\frac{1}{3000}+\frac{1}{500000}-\frac{1}{70000000}+\frac{1}{9000000000}=\text{etc.};$$

and reducing all to a common denominator, and adding the numbers into one sum (for it is not worth while going any further), then the arc will be a little greater than 518027821302775/5197500000000000, and the defect of this value from the true value will be less than the 1/1000000000000 part of the radius. For if we do not subtract the last term, 1/1100000000000, the value would be too great, and if we do subtract it, the value is less than the true value, therefore the error is less than 1/1100000000000, and thus is less than 1/1000000000000.

It is seen how exactly it comes out with such easy calculation involving only additions, subtractions and multiplications, to an extent that is not obtainable with tables. Also if the ratio of the tangent to the radius is anything else, the arc can similarly be found, and this is especially easy when it can be expressed in decimal parts. Again, since now the ratio of the circumference to the radius is given in numbers of any required degree of accuracy, by this also the ratio of a given arc to the circumference is given, and thus also the quantity of angle for a given tangent will appear with any required degree of accuracy. In this way tables may be corrected, supplemented, or, if need be, enlarged, with no great trouble. Any one who will just remember this fairly easy rule will be able without tables to attain to any required degree of accuracy with very little labor. How great an acquisition this is to geometry, I leave it to those who understand to estimate.

Critical Note.

It is difficult to see the object that Leibniz had in writing this long historical prelude to an imperfect proof of his arithmetical quadrature, unless it can be ascribed to a motive of self-praise. This suggestion would seem to be corroborated by the claims that Leibniz makes in the parts where I have quoted his own words in italics in the precis, and by the concluding sentence of the translation given in full. Even if this is so, there may be some plea of justification put forward; for Leibniz appears to have been a man impelled by many contradictory motives, but these I think can all be traced back to one origin. The time in which he lived was a time of great discoveries in geometry; Leibniz knew in his soul that he had it in him to be one of the great men in this branch of learning, but as truly recognized his great disability due to his lateness in starting, and felt that his only chance was to belong to the very exclusive set who corresponded with one another; he saw that the only way of entering this set was to do something brilliant. This may be taken as some excuse for any self-praise that we find, and to a less extent for his, to my mind, undoubted plagiarisms. With regard to the behavior of Leibniz, when charged with these plagiarisms, Sloman is not beyond calling Leibniz a liar point-blank; I prefer to call his statements perversions of the truth, made under stress of circumstances, so that *his reputation as a great and original thinker should not suffer.* For instance, to explain what I mean, I will take the statement of Leibniz to de l'Hospital that he owed nothing to Barrow. As I have said in another place, from one point of view, the point of view that Leibniz would take for the *purposes of this letter,* Barrow would be a hindrance rather than a help to Leibniz, in the formulation of his *algebraical* calculus, *after he had once absorbed all the fundamental ideas.* That is, it would seem that Leibniz always tries to tell the truth, but to put it in a form that to the uninformed reader will convey quite a wrong impression. Another example of this juggling with words and phrases is given by Sloman, in the shape of a letter from Leibniz, dated August 27, 1676, *and the first draft of the same*; these two read together are very much the same, but read apart convey a totally different impression.

A second characteristic of Leibniz may also be traced back to his desire to make up for his lateness in starting: that is, the sometimes ridiculous claims that Leibniz makes to discoveries, or rather hints at having made them. An instance is given in the *Historia* (see *Monist*, Oct., 1916, p. 599). "It is required to form the sum of all the ordinates $\surd(1-xx)=y$; suppose $y=\pm 1 \mp xz$, from which $x=2z/(1+zz)$, and $y=(\pm zz \mp 1)/(zz+1)$; *and thus again all that remains to be done is the summation* of rationals." Unless we assume that Leibniz never understood in all his life what we now call the change of the variable in integration, which to me seems rather far-fetched, the only reason why this should have been allowed to appear in a tract that was certainly written after 1712, is that Leibniz had never attempted this summation; he had set this down in 1674 and 1675 as a method of quadrature for the circle, not at that time having perceived the importance of the factor dz, or, in other words, the way in which the ordinates should be ordinated; for as I have already pointed out, at that time Leibniz could not have found dz, since he could not differentiate a product. This goes to prove that his reading of Pascal was not of the profoundest; for Pascal is very careful over this point, going to the trouble of calling the y's ordinates when drawn through the points of equal division of the base, and sines when they are drawn through the points of equal division of the arc. Probably to this characteristic is due the claim, set in italics in the manuscript above, with respect to equations of higher degrees. *He thought* he had a general method, which he had not time to verify by particular examples, and so find that his claim was erroneous. For surely this cannot be read as a claim to the Tschirnhausian transformation and the expression of a quintic in the canonical form $x^5+px+q=0$.

The date of the above manuscript is almost certainly antecedent to the manuscript that Leibniz got ready for the press, *De Quadratura*; hence his claim to be able to give examples of the calculus, except for integral powers which had already been done by Wallis, is without foundation.

With regard to the arithmetical quadrature itself, the great importance of it in the estimation of Leibniz is apparently in the correction and enlargement of tables; this claim, as Leibniz puts it, is ridiculous, although it could be so used by first constructing tables for angles whose tangents are given. But Leibniz, after giving a calculation true to twelve places of decimals, states that "the ratio of the circumference to the radius is now known," and proposes to use that. Apparently he does not see that to calculate this ratio from the series he gives, it will be necessary to take a billion or so of terms! For he does not give any hint of any modification of the series, or the use of the value obtained for some small angle.

Lastly, with regard to the calculation, it is strange that the denominator chosen as a common denominator is 15 times what it need have been; also it is a matter of wonder, considering that tables of logarithms were known to Leibniz, as a reader of Mercator and others, that Leibniz puts the matter in fractional form instead of working in decimals; thus, the arc whose tangent is 0.1 is equal to

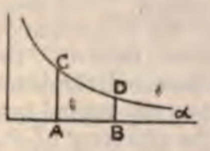
$$\begin{array}{lcl}
0.1 & - & 0.00033333333333333---- \\
0.000002 & & 0.00000001428571428---- \\
0.0000000001111--- & & 0.000000000000909---- \\
\hline
0.100002000111111--- & & 0.000333347619956--- \\
\end{array}$$

$$=0.99966865249-----$$

Finally, note that while Wallis and Brouncker are mentioned, Barrow is not. This is all part and parcel of his successful attempt to conceal, from all but Oldenburg, the fact that he had a copy of Barrow in his possession, right from the commencement of his studies.

III.

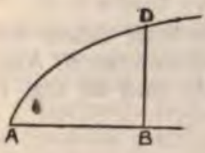
Transcribed from a manuscript tract of Newton on "Analysis by means of equations with an infinite number of terms."



AB $\sqcap x$, BD $\sqcap y$, a, b, c given quantities, m, n whole numbers. If then

$ax^{\frac{m}{n}} \sqcap y$, $\frac{an}{m+n}x^{\frac{m+n}{n}} \sqcap [\sqrt{y}] \sqcap$ area of ABD.

In connection with this the following example is to be noted:

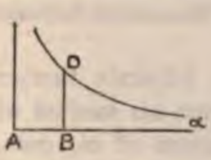


If $\frac{1}{x^2}$ ($\sqcap x^{-2}$) $\sqcap y$, that is to say, if $a=1$, $n=-1$, and $m=-2$, then we shall have

$$\left(\frac{1}{-1}x^{\frac{-1}{1}} \sqcap\right) -x^{-1} \left(\text{or } \frac{-1}{x}\right) \sqcap \alpha\text{BD},$$

produced indefinitely in the direction of α; the calculation makes this negative because it lies on the other side of BD.

Again, if $\frac{1}{x}$ (or x^{-1}) $\sqcap y$, then $\frac{1}{0}x^{\frac{0}{1}} \sqcap \frac{1}{0}x^{0} \sqcap \frac{1}{0}x^{1}$ (*this ought to be written $\frac{1}{0}$ 1*) $\sqcap \frac{1}{0} \sqcap$ infinity, which is the area of the hyperbola on either side.



If $\frac{1}{1+x^2} \sqcap y$, on division we obtain $y \sqcap 1 - x^2 + x^4 - x^6 +$ etc., and then ABCD $\sqcap \frac{x}{1} - \frac{x^3}{3} + \frac{x^5}{5} - \frac{x^7}{7}$ + etc.; or, if the term x^2 is the first in the division, the value of y will be $x^{-2} - x^{-4} + x^{-6} -$ etc., and hence BD $\alpha \sqcap -\frac{x^{-1}}{1} + \frac{x^{3}}{3} - \frac{x^{-5}}{5}$ + etc.

The first method is to be used when x is small enough, and the second when x is large enough.

Gerhardt then remarks that Leibniz has noted completely the following two cases of extractions of roots:

$$(\surd a^2 + x^2) \sqcap y, \text{ and } \frac{\surd(1+axx)}{\surd(1-bxx)} \sqcap y.$$

Gerhardt further notifies the reader that he has omitted everything that he has found Leibniz to have copied out word for word, on comparison with Biot's edition of the *Commercium Epistolicum* (1856).

In the above, Leibniz marks interpolated remarks of his own with either [ ] or (* *).

In the same manner, Leibniz has written out word for word the part of the manuscript dealing with the solution of adfected equations (against this he has put the final observation: "And these things that have been given will be sufficient for the investigation of areas of curves"), in addition to the part which follows, "the application of what has been given to other problems of the same kind," which, as being already known to him, he has not copied out. He goes straight on to the next section, "To find the converse of the foregoing, that is, to find the base when given the area, and to find the base when given the length of the curve." He has written this out word for word; also he has noted fully to the end the "proof of the method of solution of adfected equations."

At the end of these extracts from Newton's tract follow the words, "I extracted these things from the letter of Newton 20 Aug. to Newton." Gerhardt states that he has already said all that is necessary about the contents of these extracts.

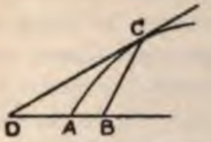
SECOND SHEET.

Extracts from the correspondence between Collins and Gregory.

Among a number of partly illegible and unintelligible notes the following were to be noticed.

Gregory, January, 1670: Barrow shows himself to be most subtle in the geometry of optics. I think that he is superior to all whose works I have looked into, and I esteem this author beyond anything that can be imagined.

Sept., 1670: I think that Barrow has gone infinitely further than all those who have written before him. From his method of drawing tangents, combined with certain meditations of my own, I found a general geometrical method of drawing tangents, without calculation, to all curves, which not only contain his particular methods, but the general method as well. This is shown in 12 propositions.



Letter of Newton, 1672: ABC is any angle, AB$\sqcap x$, BC$\sqcap y$. Take, for example, the equation,

$$\underset{3}{\overset{0}{x^3}} - 2\,\underset{2}{\overset{1}{x^2y}} + \underset{2}{\overset{0}{bx^2}} - \underset{1}{\overset{0}{b^2x}} - \underset{0}{\overset{2}{by^2}} - \underset{0}{\overset{3}{y^3}} \sqcap 0.$$

Multiply the equation by an arithmetical progression, both for the second dimension y and for x; the first product will be the numerator, and the other divided by x will be the denominator of a fraction which will express BD, thus.

$$\mathrm{BD} \sqcap \frac{-2\,x^2y + 2\,by^2 - 3\,y^3}{3\,x^2 - 4\,xy + 2\,bx - b^2}.$$

Moreover that this is only a corollary or a case of a general method for both mechanical and geometrical lines, whether the curve is referred to a straight line, or to another curve, without the trouble of calculation, and other abstruse problems about curves, etc. This method differs from that of Hudde and also from that of Sluse, in that it is not necessary to eliminate irrationals.

NOTE.

It is almost useless trying to write a critical note on the above in such an incomplete state. But I may remark that Leibniz apparently was at the time quite ignorant of what we now term "putting in the limits for a *definite* integral."

Gerhardt considers that the existence of this extract proves conclusively that Leibniz did not see the letter of Newton so often referred to; forgetting, as Sloman remarks, that Leibniz ought not to have seen the tract at all!

P. S. In allusion to footnotes 3 and 18, with regard to the use of the word "moment" or "momentum" in the sense used by Leibniz, I have found (since the above was written) that Cavalieri, in his *Exercitationes Sex,* defines the term in the mechanical sense and gives much of the matter of Pascal on Centers of Gravity, as it appears in the "Letters of Dettonville." I suggest that Leibniz saw it in Cavalieri, and that its origin is to be traced to Galileo. J. M. C.

OUR MUSICAL IDIOM.

WITH AN INTRODUCTION BY GLENN DILLARD GUNN.

INTRODUCTION.

The effort to expand the means of musical expression is as old as the art itself. It is recorded in each chapter of musical history; it has been interrupted only during those periods of the art's development wherein the composer has been concerned with the completion of art-types already defined.

Every advance in the art has been prefaced by a period of experimental effort, which has sought new modes of expression. So soon as these modes of expression have been defined and their tendencies and the laws governing them have been apprehended, experiment has been replaced by careful conformance to law and tradition, which has operated to the perfection of the new art type.

The present is preeminently an epoch of experimentation. The old art types have been completed. The harmonic vocabulary based upon the sequences of tonality established in these completed art forms also has been exhausted and for the past half century composers have been concerned with the development of new harmonic idioms. (Viz., Liszt, Wagner, Strauss, Franck, D'Indy, Debussy, Ravel, Shoenberg, Busoni.) As these composers have discovered and employed new harmonies, new scales and new sequences of tonality with their resultant new harmonic progressions, the theoretician has endeavored to classify their discoveries according to his established system with results weirdly confusing. The crying need of the moment seems to be a new system for the naming and classifying of all possible tonal combinations.

"Harmony is that which sounds together," wrote Bernard Ziehn twenty-five years ago. But the average theoretician comprehends only those simultaneously produced sounds which may be arranged in series of superimposed thirds. In the meantime the composer has consciously employed many harmonies which are not formed of superimposed thirds. (Viz., Debussy's major second as the first interval of the tonic chord, or Shoenberg's combinations of superimposed fourths, to cite familiar examples.) The executive artist, upon whom the composer is dependent for the delivery of his message, is, in turn, dependent upon the theoretician for a logical classification of the new harmonies. The composer of the present is almost equally dependent upon some scientific classification of his material. Naturally the public has first looked to him for this classification. But he seems able to make it only for himself, as Reger and Shoenberg have done. In any event the world has been slow to adopt these special classifications and is still seeking a general system that will include all possible harmonies in logical order.

That system has been evolved by Mr. Ernst Lecher Bacon of Chicago, who by applying the principles of algebraic permutations to the problem has succeeded in formulating all harmonies that may possibly exist in the present system of twelve tones (of itself a most important service) and having formulated them, has found a system of nomenclature which actually describes any possible combination of tones and makes a general or special classification possible.

The value of this new system of nomenclature to the executive artist is immediately apparent. That puzzled individual may name and classify the new tonal combinations which he is required to memorize and present convincingly to the public. The composer is even more importantly served by Mr. Bacon's researches. For he is shown at a glance all possible harmonies (there are but 350) and all possible scales (of which there are about 1490). He may select from the clear and concise tables placed at his disposal those harmonies and scales which seem to him useful and beautiful and having familiarized himself with their color and feeling, in short, made them a part of his own consciousness, may employ them subjectively to the expression of feeling and sensibility, to the building up of his own especial harmonic idiom. For though the composer's work is best when it is most subjective, he is constantly obliged to concern himself with the facts of his art and out of these facts to fashion that delicate fabric of feeling and fantasy which is to give freer and fuller powers of expression to the music of the future.

GLENN DILLARD GUNN.

THE FORMATION OF SCALES.

The chromatic scale has become established as the basis of modern harmony. Though the major and minor modes are still accorded that recognition which the printed key signature seems to imply, modern compositions so bristle with accidentals that even to the eye, and still more to the listening ear, is it evident that the restrictions of the major and minor system have been destroyed.

However, few of our modern composers treat the chromatic scale purely in itself; the intrusion of other scales which are synthetically formed from the chromatic scale is always felt. The chromatic scale is only the analytic product of others, which consist of combinations of its semitones, wherein intervals are found which are collections of semitones. Of these synthetically formed scales there are many in number but few in use; and each may form a separate harmonic basis. Beethoven and Liszt, the latter more notably, occasionally used scales differing markedly from the major and minor; but their appearance was only incidental, and the scales were rarely made use of as bases for harmonic systems. Busoni created, as he says, 113 different scales through rearrangements or permutations of the intervals of the major and minor scales.[1] Debussy has used a few unfamiliar scales, notably the whole tone scale, for a thorough harmonic basis. However, as will be shown, a vast number of scales that have never before been conceived are opened to discovery through the application of the principles of algebraic permutation to arrangements of tonal sequences.

A *scale* is a series of ascending or descending tones. Such a series may conform to a pattern, a regularly recurring succession of intervals in certain order, bounded by a fixed interval; or it may not conform to pattern. The pattern may or may not conform to the duodecimal system. If the scale conform to pattern it must be bounded by a fixed interval. Intervals of simple physical ratio are preferred, and these are found, slightly tempered, in the duodecimal system.

Until now, the octave only has been consciously used as a fixed interval, but there is no reason why, in specialized cases, other intervals could not exist between corresponding tones in recurrences of the pattern. We may have scales repeating at each fourth, fifth, sixth, seventh, ninth, tenth, etc. But because we are at present engaged in a classification of scales which incidentally involves the discovery of a multitude of unheard-of ones, and because a classification of such scales as these would be of formidable length, we must be content to study that most important class of scales in which each succeeding repetition begins an octave above or below the preceding one.

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[1] His figures are incorrect as, mathematically computed, the number of permutations of the combination of intervals is: (*a*) of the major scale, 21; (*b*) of the minor scale, 140.

Again we must distinguish between two classes of scales whose basis is the octave. The first class is that one in which the smallest scale-units in the octave number 12; this is our *duodecimal* system. The second class contains many systems, in each of which the number of the smallest units is either greater or less than 12. We will consider both of these classes, for in the consideration of the first class we can enlarge considerably the present scope of the duodecimal system, while in the consideration of the second class we may discern dimly certain possibilities of the future. First we will discuss the scale possibilities of the duodecimal system.

By a division of the octave into twelve parts the common *chromatic* scale is formed. Now by grouping together certain of these twelfths of an octave, the so-called "semitones," we may form scales whose gradation is uneven and less refined than that of the chromatic scale. If we are given a certain combination of intervals which, added together, give the octave, we can permutate these in a number of different ways; that is, we can rearrange the given intervals to form different scales. We also may have combinations in which the same intervals occur more than once. If n is the number of intervals between octaves in the scale and n_1 of them are alike, and n_2 others are alike, etc., the number of scales (P) that can be formed by permuting the given combination of intervals is:

$$P = n!/n_1!n_2!\ldots\ldots$$

(The exclamation point, read "factorial," denotes that the number which it follows is a product of all integers less than and including itself, each integer being a factor only once.)

For example, we desire to find the number of scales that can be formed with the intervals of the major scale. The major scale consists of 5 whole tones and 2 half tones, making a total of 7 intervals.

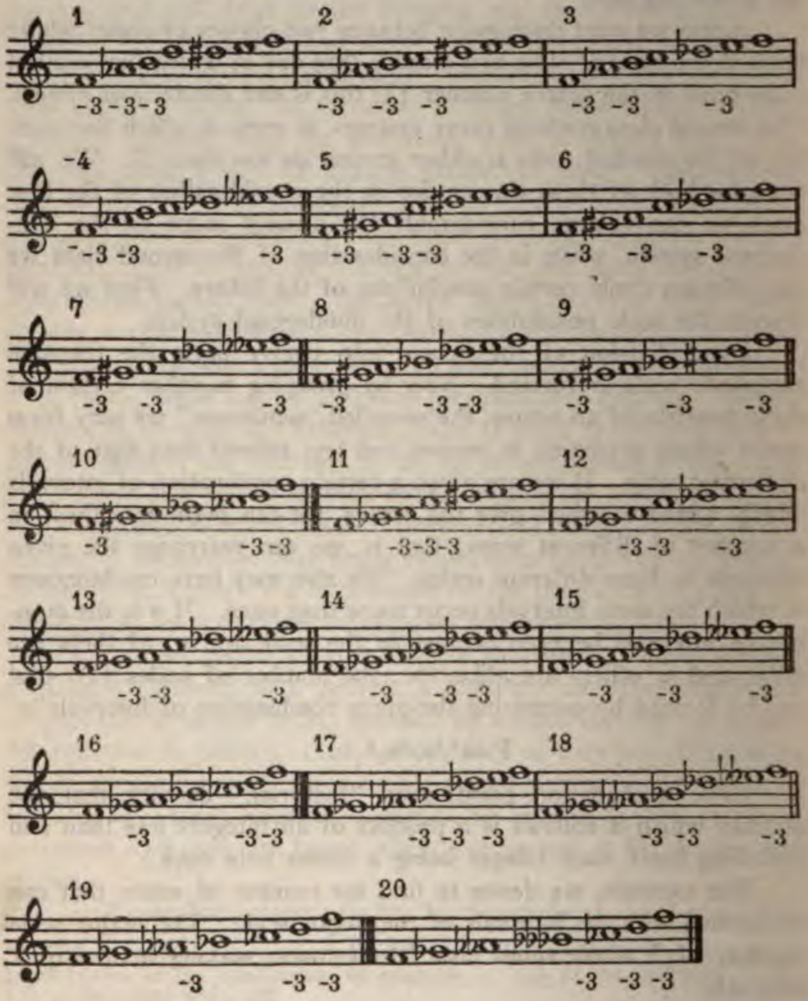
Thus $P=(n!/n_1!n_2!)=(7!/5!.2!)$

$$=[1.2.3.4.5.6.7/(1.2.3.4.5.)\,(1.2)]$$

$$P=42/2=21.$$

As an example of the way in which all possible scales can be formed out of a certain combination of intervals, 20 scales will be formed out of the combination (three minor thirds and three minor seconds) as follows:

The symbol (—3) will be written below respective minor thirds.



It will be observed that a new series begins with each double or triple bar.

A triple bar is written before each chromatic elevation of the lowest minor third.

A double bar is written before each chromatic elevation of the middle minor third.

The uppermost minor third always starts at its lowest possible position and is raised successively to its highest possible one, after which a change is made in the relative position of the lower minor thirds.

This method of forming all scales from a certain combination of intervals is purely arbitrary.

Now it is possible to form a great number of combinations in which the sum of the intervals is an octave. Moreover, as we have seen, usually a number of scales can be formed out of each combination. Each scale is to be considered a permutation of the combination's intervals.

In the following table will be found every combination possible with intervals as small as the minor second and not greater than the major third. Intervals larger than the major third are not used because in the formation of scales they would make gradation too abrupt and uneven. The table will also include calculations of the number of permutations (to be regarded as the number of scales) possible with each respective combination, according to the formula. The vertical columns contain the intervals minor 2d, major 2d, minor 3d, major 3d, respectively. The horizontal rows of numbers are the combinations. A number (n) falling in a vertical column (v) means that the interval (v) is repeated n times in the combination in which n lies (see Table I).

To make the function and construction of the table more plain two of the combinations may be explained. Combination 1 indicated by the number in the extreme left-hand column, consists of twelve semitones. It is therefore a formula of the chromatic scale, and has therefore only one permutation. Combination 21 contains two minor seconds, two major seconds and two minor thirds. From it may be formed fifteen scales or permutations.

Means have now been shown to find all possible scales in the twelve-tone system, scales which have intervals exceeding the major third in size being omitted. Adding the number of permutations formed with all combinations a total of 1490 scales is found.

A systematic study of these 1490 new scales would lead to the discovery of many valuable scales. I have found many that are interesting by this method, but will mention only a certain class of these scales, which I will call *equipartite* for want of a better name.

TABLE I.

	Combinations				Permutations			Combinations				Permutations	
	Minor Seconds	Major Seconds	Minor Thirds	Major Thirds	Calculations	Permutations		Minor Seconds	Major Seconds	Minor Thirds	Major Thirds	Calculations	Permutations
1	12				P12!/12!	1	18	3		3		6!/3! 3!	20
2	10	1			11!/10!	11	19	2	5			7!/2! 5!	21
3	9		1		10!/9!	10	20	2	3		1	6!/2! 3!	60
4	8	2			10!/2! 8!	45	21	2	2	2		6!/2! 2! 2!	90
5	8			1	9!/8!	9	22	2	1		2	5!/2! 2!	30
6	7	1	1		9!/7!	72	23	2		2	1	5!/2! 2!	30
7	6	3			9!/3! 6!	84	24	1	4	1		6!/4!	30
8	6	1		1	8!/6!	56	25	1	2	1	1	5!/2!	60
9	6		2		8!/2! 6!	28	26	1	1	3		5!/3!	20
10	5	2	1		8!/2! 5!	168	27	1		1	2	4!/2!	12
11	5		1	1	7!/5!	42	28		6			6!/6!	1
12	4	4			8!/4! 4!	70	29		4		1	5!/4!	5
13	4	2		1	7!/2! 4!	105	30		3	2		5!/2! 3!	10
14	4	1	2		7!/2! 4!	105	31		2		2	4!/2! 2!	6
15	4			2	6!/2! 4!	15	32		1	2	1	4!/2!	12
16	3	3	1		7!/3! 3!	140	33			4		4!/4!	1
17	3	1	1	1	6!/3!	120	34				3	3!/3!	1
												Total	1490

An *equipartite* scale is one in which the same pattern of intervals is repeated an integral number of times within the octave. If a scale is *bipartite* a group of intervals will appear twice within the octave with no remainder; if the scale is to begin on F its two parts begin, respectively, on F and B. As a result in this case it is immaterial whether the tonic is B or F, for the scale sounds alike either way, except for the transposition.

We may split the sum of twelve semitones (semitones being regarded as intervals) into two parts or three. Dividing it into two parts, each part containing six semitones, allows us again to divide this semi-octave into two or three parts. Dividing the octave into three parts, each part has four semitones, which may again be divided by two. Thus we may split the octave into 2, 3, 4, and 6 equal parts. Scales formed by such divisions may be called, respectively, bipartite, tripartite, quadripartite, and sexpartite. As the

TABLE II (FOR BIPARTITE SCALES).

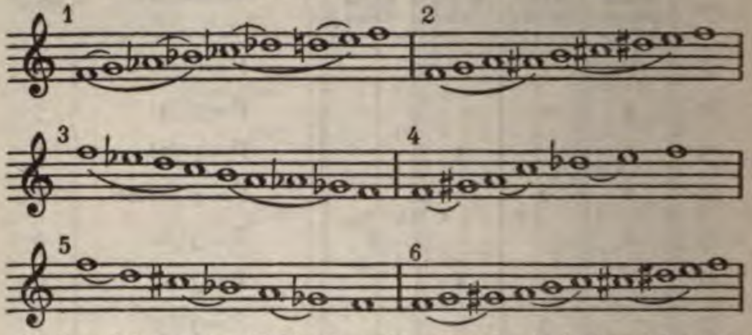
COMBINATIONS					PERMUTATIONS	
No.	1 MINOR SECOND	2 MAJOR SECOND	3 MINOR THIRD	4 MAJOR THIRD	CALCULATIONS	NO. OF PERM.
1	6				$P=6!/6!$	1
2	4	1			$P=5!/4!$	5
3	3		1		$P=4!/3!$	4
4	2	2			$P=4!/2!\ 2!$	6
5	2			1	$P=3!/2!$	3
6	1	1	1		$P=3!$	6
7		3			$P=3!/3!$	1
8		1		1	$P=2!$	2
9			2		$P=2!/2!$	1
					Total	29

TABLE III (FOR TRIPARTITE SCALES).

COMBINATIONS					PERMUTATIONS	
No.	1 MINOR SECOND	2 MAJOR SECOND	3 MINOR THIRD	4 MAJOR THIRD	CALCULATIONS	NO. OF PERM.
1	4				$P=4!/4!$	1
2	2	1			$P=3!/2!$	3
3	1		1		$P=2!$	2
4		2			$P=2!/2!$	1
5				1	$P=1!$	1
					Total	8

last two types may be classed under the first and second they do not require a separate classification. In Tables II and III the combinations in the bipartite and tripartite types are given: in other words, the possibilities of combinations with six and four semitones, respectively, are shown. Each arrangement of a combination is then repeated in the remaining half or two-thirds of the octave.

A few interesting equipartite scales are herewith shown:



(1, 2 and 3 are from Table 2, combination No. 4; 4, 5 and 6 are from Table 3, combination No. 3.)

Scales formed by permutating combination No. 4 in Table II, and combinations Nos. 2 and 3 in Table III are especially interesting. No. 1 is formed by alternating major and minor seconds, while No. 3 is formed in the same way, except that in it the order of the intervals of No. 1 is reversed. Even such a mechanically formed scale as this sounds beautiful and original. It is a noteworthy fact that in scales 1 and 3 the chords formed on every degree are diminished. Scales Nos. 4 and 5 are built similarly; only a minor third and a minor second alternate. Chords formed on every degree of these scales are augmented.

SCALES FORMED FROM SYSTEMS OTHER THAN THE DUODECIMAL.

Although to-day the importance of systems containing other intervals than multiples of semitones is questionable, it is nevertheless interesting to know that such systems may be exploited for scale and harmonic possibilities in the same manner as our present system. Busoni has already experimented with the *tripartite* tone scale; that is, a scale in which each whole tone is divided into three instead of two whole parts. The physicist may scorn the idea of a new system, knowing that the duodecimal system contains the simplest physical intervals, yet it must be remembered that the perfect intervals are also not found in the 12-tone system, because of "tempering." Moreover in the other systems many of the most important intervals of the duodecimal system will be duplicated. Although probably no system will ever be of equal importance with the duodecimal, it is not inconceivable that, just as certain new scales within our present system have been chosen by recent composers as harmonic and melodic idioms of expression, so certain "foreign" systems may once be chosen for similar purposes.

Accordingly, we are to consider any equal divisions of the octave. However, certain divisions, as for example into 11 or 13 equal parts, are not of importance, since the intervals formed in this way would only be confounded with poorly tuned intervals of the 12-tone scale. In order to discriminate in the selection of numbers with which to divide the octave it is well to choose only those numbers which are multiples of the smallest prime numbers, 2, 3, and 5. We may call each of these systems an "N-tone chromatic system." If the system is one in which the number of smallest intervals is 9, we may call it a 9-tone chromatic system. We are not bound to confine the use of the term "chromatic" to our duodecimal system, since in its musical application the word is used to describe a succession of the smallest possible intervals.

In considering the N-tone chromatic systems we may go through the same steps through which we have passed in considering the duodecimal system. In each of these unfamiliar systems there are chromatic intervals which may be combined and permutated to form scales of more rapid and uneven gradation. Just as before, we have to set a certain limit to the size of an interval employed in one of these scales. In the five-tone system a coupling of only two chromatic intervals produces an interval almost too great to exist in a scale of moderately refined gradation. In the 24-tonal system a coupling of as many as 6 intervals into 1 is acceptable. It will readily be seen that to construct tables for all N-tonal systems through which an infinite number of gradations is possible, would require much space. It has already been stated that those systems having numbers of chromatic intervals equal to multiples of 2, 3 and 5, are most important. They are systems of 4, 5, 6, 8, 9, 10, 12, 14, 15, 16, 18, 20, 21, 22, 24, etc., chromatic tones; for demonstration I will select only 2 of these; namely 8- and 9-tone systems.

In the tables that follow I will give the number of combinations and calculate the number of permutations for each combination with the selected N-tonal systems. In other words, scale possibilities with systems having 8 and 9 tones will be shown in each respective table.

TABLE II, N=8

	Combinations			Permutations	
	$\frac{1}{8}$ Octave	$\frac{2}{8}$ Octave	$\frac{3}{8}$ Octave	Calculations	Permutations
1	8			8!/8!	1
2		1		7!/6!	7
3	5		1	6!/5!	6
4	4	2		6!/2! 4!	15
5	3	1	1	5!/3!	20
6	2	3		5!/2! 3!	10
7	2		2	4!/2! 2!	6
8	1	2	1	4!/2!	12
9		4		4!/4!	1
10		1	2	3!/2!	3
				Total	81

Intervals used do not exceed $\frac{3}{8}$ octave.

Number of intervals corresponding to Duodecimal System=4.

TABLE III, N=9

	Combinations			Permutations	
	$\frac{1}{9}$ Octave	$\frac{2}{9}$ Octave	$\frac{3}{9}$ Octave	Calculations	Permutations
1	9			9!/9!	1
2	7	1		8!/7!	8
3	6		1	7!/6!	7
4	5	2		7!/2! 5!	21
5	4	1	1	6!/4!	30
6	3	3		6!/3! 3!	20
7	3		2	5!/2! 3!	10
8	2	2	1	5!/2! 2!	30
9	1	4		5!/4!	5
10	1	1	2	4!/2!	12
11		3	1	4!/3!	4
12			3	3!/3!	1
				Total	149

Intervals used do not exceed $\frac{3}{9}$ octave or a major third, as translated.

Number of intervals corresponding to Duodecimal System=3.

The Numbers of Tones and Intervals Found Correspondingly in Any Two N-Tonal Systems.

If we choose a common tonic for all N-tone chromatic scales we will find certain other tones which are common to two or more of these scales. For example, if we form both a 9-tone chromatic and a duodecimal scale upon C, we will expect to find two tones in common besides the C and its octave. They will be E and G sharp; for each of these tones marks the partition of the octave into three equal parts. This means that certain intervals in one system are the same as intervals of another. But an interval common to two systems cannot be the same multiple of the smallest unit in each system. If we desire to find the number of intervals which are found correspondingly in each of the two systems, we need merely to find the largest factor common to the number of chromatic divisions of both systems. For example, to find the number of intervals which are common to the 18-tone and the 12-tone chromatic scales we find the G. C. F. of 18 and 12, which is 6. This is the desired number. Of course, intervals which are multiples of this common interval (the whole-tone, in this case) are also common to both systems.

Intervals of N-Tone Chromatic Scales.

Throughout our entire treatment of scale possibilities there is one interval which remains constant; namely, the octave. The ratio of this interval, that is the ratio[2] of the vibration frequency of the higher tone to that of the lower tone is always 2. If N is the frequency of the lower tone, its octave is 2 N. Now N and 2 N may be written as 2^0 N and 2^1 N respectively, since any quantity with an exponent 0 equals unity. It is evident that the frequencies of any tones between $N \times 2^0$ and $N \times 2^1$ can be expressed as N times the coefficient 2 with an exponent varying between 0 and 1.

If the octave contains *r* equal intervals, the difference between 0 and 1 of the exponent of 2 will be divided into *r* parts. This is true because (*a*) equal intervals form equal ratios of vibration; and (*b*) equal ratios may be expressed as the quotients of a constant in which the difference of the constant's exponents in the numerators and respective denominators remains constant. To illustrate:

$$2^1/2^0 = 2^{1-0} = 2$$
$$2^6/2^5 = 2^{6-5} = 2.$$

Hence $2^1/2^0 = 2^6/2^5$.

Thus

$$2^{\frac{1}{r}} \text{ or } \sqrt[r]{2}$$

expresses the ratio of any interval formed by two adjacent tones in an equally tempered scale of *r* intervals. Moreover the intervals which any tonic (arbitrarily chosen in the case of the equally tempered scales) forms with the successive ascending tones above it, are, respectively:

$$2^{1/r}, 2^{2/r}, 2^{3/r}, 2^{4/r}, \ldots\ldots\ldots 2^{(r-1)/r}, 2^{r/r} \text{ or } 2.$$

---

[2] This ratio is physically defined as the interval itself.

From these facts we derive two general formulas: (A) expressing the physical interval or vibration ratio between 2 tones and (B) the vibration frequency of any tone lying above a given tone, N.

(A) $\text{I (interval)} = 2^{c/r}$,

(B) $\text{V (vib. freq.)} = \text{N} . 2^{c/r}$,

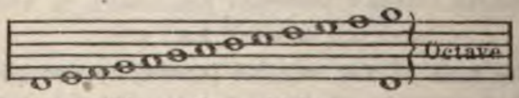
where r = the number of chromatic intervals per octave, in the given system; and c = the number of chromatic intervals separating the two tones whose physical ratio is to be found.

With these formulas we can express the various intervals of any equally tempered scale.

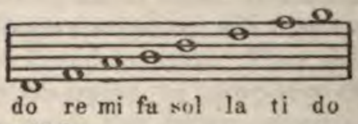
NOTATION OF SCALES.

In considering the great number of scales of which we have learned in the previous section we are confronted with the problem of their notation. Our present notation is really suited for seven scales only; namely, the major scale and the scales formed by cyclically rotating the permutation of the intervals of the major scale, that is, the Dorian, Phrygian, Lydian, Mixolydian, etc. We cannot write even a minor scale without the use of an accidental. Then with regard to the 1483 other scales, because of this great number and variety, we cannot do more than make general statements.

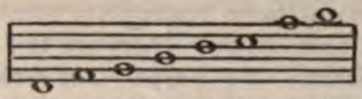
We realize, to begin with, that the ideal notation of our present-day music should be one which is designed to eliminate the inconveniences of accidentals. Such a notation would be naturally one designed from the chromatic scale; and because the chromatic scale contains all of the 1490 other scales of the duodecimal system, it would be adaptable, in a perfect sense, to all of these scales. We could accomplish the notation of the chromatic 12-tone scale with a six-line staff giving each degree a separate line or space, as shown:



The major scale on this staff would be:



The minor scale would be:



The mental picture we obtain of the relation of the intervals of these two scales in this manner is alone an advantage. Furthermore, in the six-line staff notation we are less bound to avoid deviations from our chosen scale; we are freer to escape from the tyranny of sharps and flats. An abhorrence of accidentals has always tied us to our chosen scales. Other advantages of this notation could be cited, but the chief one is, of course, that merely through the addition of another line (which does not confuse us optically) we are able entirely to avoid accidentals.

However, the difficulty of introducing this system into common use would be almost too great to be overcome. An attempt at this could be likened to the recent attempts at introducing a universal language; for were we all to learn a universal language we would still have to retain a knowledge of the old for its literature. We are therefore compelled to adjust our new scales to the common notation of the five-line staff.

We may eliminate from consideration not only the major scale and those scales formed by a *cyclic rotation*[8] of its permutation of intervals, but also the minor scale with its corresponding scales formed by a similar cyclic rotation. This suggests to us a process that will greatly simplify our whole problem. We see that the notation for one scale is suitable for all other scales formed by a cyclic rotation of the permutation of its intervals. The number of these scales will depend upon the number of tones or intervals in the original one. The notation for a scale of n tones or n intervals will serve for $(n-1)$ cyclically related scales. Thus one notation serves for n scales.

We realize that out of a certain combination of intervals we may form more than one cyclic group, for some combinations have as many as 168 scales while in no cyclic group can there be more than 11 different scales. A formula with which we may calculate the number of cyclic groups in each combination is:

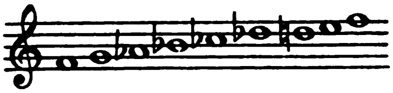
$$G=(n-1)!/n_1!\,n_2!\,n_3!\ldots.$$

where n is the number of intervals in a combination, and n_1, n_2, n_3, etc. are the numbers of times respectively which certain intervals are repeated in the combination. There are few exceptions to this formula, all of which are of one type. The erroneous type is that in which $(n_1+n_2+n_3+n_4\ldots)$ exceeds $(n-1)$. These exceptions often cause fractions which cannot be integrally expressed. In cases of this exception we must find our number of cyclic groups by actual trial. But if we have found one signature suitable for each whole cyclic group of scales we have, in general, shown only one-twelfth of possible signatures, for in most cases a different signature is necessary for each chromatic degree. Only in equipartite scales are fewer signatures than twelve necessary to each group. If a scale is bipartite only six signatures are necessary; if tripartite, four; if quadripartite, three; and if sexpartite, two.

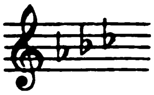
---

[8] The term defines itself. A *cyclic rotation* of a permutation is one in which the terms are always written in the same order, but each successive permutation begins with the second term of the preceding one. The following is a *cyclic* group of permutations: A B C D, B C D A, C D A B, D A B C.

As we are considering these 227 scales representing cyclic groups primarily for their notation, we are confronted with the question, what signature shall we give to a work based on a scale like the following?



None of our conventional signatures for major scales will apply to this scale; for we see the three essential signatures are:



d flat being unnecessary as a signature because it is cancelled immediately, the scale being an 8-tone scale, which necessitates the repetition of one note.

We will find that most of the scales, like this one, will require signatures other than those which we have employed for our major and minor modes. Consequently we will not try to reconcile our customary signatures with those natural to the new scales. Therefore, in order to make a signature for any scale on any degree, write down those accidentals which appear in the notation of the scale, omitting those accidentals only which are cancelled as the scale continues. We may rightly call this a *natural* system of signatures.

Concerning the method of finding each scale representative of a cyclic group for a given scale degree, the following means are perhaps the simplest:

1. Choose an interval which occurs singly in the combination and place it in the lowest position in the scale.
2. Permutate the other intervals above it in every possible way.
3. Each permutation, with the first interval remaining in a fixed position, will form a desired scale.
4. When no interval occurs singly in the combination there is no rule which applies generally; but because of the small number of combinations of this character the desired scales can be easily found by trial.

There is little need for investigating the problems of notation of N-tonal systems until such systems come into use. Solutions to such problems are really simple and arbitrary. Suffice it to say, there is no need of retaining the five-line staff for N-tonal notation. It would be unfortunate if one were compelled to read a totally new system of intervals from a staff with which one would constantly associate accustomed intervals.

Although it may seem strange that so much attention is paid a subject like the formation of scales, there is nevertheless justification in an investigation of this sort. A scale has far greater importance than the mere sequence of tones comprising it would imply. Practically all of the hundreds of melodies we know can be formed, almost without accidentals, from the major and minor scales. Virtually all of the common harmonies can be constructed from these modes. The vast amount of musical thought and feeling has until recently expressed itself in major and minor. But the chromatic scale offers a much wider field of expression: for it contains not only the major and minor scales, but over fourteen hundred others. Nevertheless, although the chromatic scale has become the basis for modern harmony, melody does not seem to flow freely chromatically. Our musical speech continually demands some simple group of tones and larger intervals. Without some limitation more binding than the chromatic scale, we are helplessly confused with the wealth of possibilities. Such limitations are found among the multitude of scales derived synthetically from the chromatic.

Debussy and some of his colleagues have made their idiom or "dialect," as it were, the whole-tone scale. This one scale, because of its uncertain "tonality," and its "color," has been the outstanding characteristic of the French impressionists.

A few other scales, such as the Greek "modes," which are all cyclically related to the major scale, have been the basis for numerous works. On the whole, there is no reason why other scales, among the vast number shown to exist, should not become equally important idioms of expression.

Limitations of space unfortunately prevent me from tabulating completely the fourteen hundred and ninety scales of the duodecimal system.

Concerning the N-tone scales, it is well to consider for illustrative purposes the words of Busoni in regard to his tripartite tone scale:[4] "The tripartite tone," says he, "has for some time been demanding admittance, and we have left the call unheeded." With the tripartite tone he encounters a difficulty which will be found also in considering other N-tone scales. He says we would lose through the tripartite tone the minor third and the perfect fifth. Now a chromatic scale in which the most important intervals do not occur (intervals whose ratios are expressed as quotients of the smaller integers) will never form quite as valuable a system as a chromatic scale that contains them. Realizing this, Busoni has attempted to reconcile the 12-tone with the 18-tone system; that is, a system of bipartite tones with one of tripartite tones. His solution is naturally a 36-tone scale involving the sexpartite tone. To entertain any hopes for a system of sexpartite tones seems to me futile. A system of 24-tone chromatics might be better reconciled with our duodecimal system. This example merely shows us that we cannot attempt to reconcile the N-tonal systems with each other or with the duodecimal system. An 18-tone chromatic system is perhaps next in importance to the duodecimal system, but it is comprehensive and important enough in itself, even though it does not contain minor thirds and perfect fifths.

Again we must consider how we are to produce these tones, as Busoni has mentioned in regard to his tripartite tone scale. For experiment and a training of the ear to the tripartite tone Busoni recommends Dr. Thaddeus Cahill's dynamophone, an instrument which would, however, be very difficult to obtain or to construct. A Seebeck's siren with a special disk for each system would be a good substitute. The number of holes in each circular row could be mathematically computed with the help of the formulas:

$$I = 2^{d/r} \text{ and}$$
$$V = N.2^{d/r},$$

---

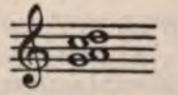
[4] For Busoni's statements read his *Sketch of a New Esthetic of Music,* New York, 1911.

which are explained in previous pages. A motor to revolve the disk would furnish a constant speed of rotation.

Such experiments would furnish means of acquiring a sense of intervals other than those to which we are accustomed; but, in Busoni's words, "only a long and careful series of experiments and a continued training of the ear can render this material approachable and plastic for the coming generation, and for art."

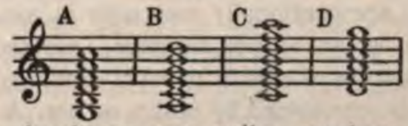
A NEW HARMONY.

Our present system of harmony, the system of chords (harmonies formed of superimposed thirds), is deficient in two important respects. First, it is often unwieldy; and second, it is not fully comprehensive. This latter shortcoming is partly responsible for the former, since it is true that we may represent certain harmonies, seemingly not within the scope of our system, in complex ways. To illustrate: let us examine the various unsatisfactory ways of describing the simple harmony:

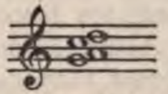


If the harmony is a chord, we must be able to build it up by superimposing thirds. But no complete chord exists that contains each of these and only four notes.

But there are chords containing more than four notes which contain the notes of the harmony, such as the following:



From these chords we may strike out those notes foreign to the harmony and derive what we call an *incomplete* chord. The harmony:



may therefore be termed an *incomplete 11th chord* (as in A, B, or D) or an *incomplete 13th chord* (as in C). If we are willing to recognize an incompleteness of this sort as mathematically rigid, we must still admit that such a naming of the harmony as an incomplete 11th does nothing more than justify its existence among chords. It does not name the harmony for there are innumerable incomplete 11ths. To name the 11th chord in each case is difficult, the general method being that of determining upon what degree of the major or minor scale it is built. But, again, must we consider all harmonies in the light of the major or minor scales to-day when many other scales are being used? Furthermore we must find where the incompleteness lies. Lastly one would suppose that every harmony has one fundamental position, but here are four. One should be able to tell what sort of inversion of the fundamental harmony the one in question is. How is this possible when the harmony in its position is a different inversion with each fundamental?

If we allow ourselves the latitude of recognizing diminished thirds, we may say the harmony is composed of two diminished thirds separated by a minor third. Taking this liberty we might have a *specialized chord* or so-called *altered chord*, but how shall we describe any particular one? Moreover, it is false to assume that diminished thirds are thirds at all; for they are seconds.

Sometimes, if the harmony is preceded or followed by others, we may analyze it under our present system by considering certain tones as "passing tones," "suspensions," "afterbeats," "syncops," "organ-point," etc. The awkward system of figured bases sometimes affords a means for expressing simpler chords.

If such is the fate that a simple harmony like the above suffers in analysis, what lot befalls the multitiude of more complex harmonies? The best that modern analysis can do for them is to treat them in relation to surrounding harmonies. Even then, "unresolved suspensions," etc. are continually met with in modern music. If harmony is "that which sounds together," we should be able to define any combination of simultaneously sounding tones, whether this combination is surrounded by others or not. A note suspended from a consonant to a dissonant chord is sounding in the second as well as in the first harmony. Does not an organ-point form a separate harmony with each of a series of chords "moving through it," even though these chords are dissonant with the organ-point? A harmony is a harmony whether dissonant or consonant. Yet of the vast majority of dissonant harmonies few can be adequately named and classified in themselves.

The chord system is adequate in analysis of older works only. It can give only a superficial analysis of modern works.

A more important objection even than that of inadequate nomenclature is that by reason of our use of the chord system we are hindered in enlarging our scope of harmonies. The conception of harmonies given us by this system restrains us from enlarging our harmonic vocabulary. Bred in the chord system, we are prone to regard any harmony which is not chordic in construction as a mere variance of some "simple" chordic form. Many a stereotyped theorist would shudder at the notion of giving the above harmony a prolonged and separate existence. It must be immediately resolved into a stable form; the tonic triad of G major, etc., etc. Are we blind to the existence of harmonies not made up of superimposed thirds? Shall we refuse to recognize non-chordic harmonies merely for the technical reason that we employ a system of superimposed thirds, which was an expedient solution to theoretical problems two centuries ago? Because of the limitations of our present system, a vast number of harmonies remain to-day virtually undiscovered. Although many have been employed passingly and subconsciously, few have been employed deliberately, few are spoken of as a part of the composer's vocabulary.

Fully realizing the importance of the chord system in the analysis of older works (for these were conceived in the spirit of the chord system), I believe it is important that a new and fully comprehensive system should supplement it, a system that would prove adequate for the analysis of modern writings. Whereas the old system embraces chords only, the new should embrace all harmonies. The chord scheme would then take its place as a sub-system of the more general and all-inclusive system.

Just as the present method is more than a mere scheme of nomenclature, so the one which I propose should be considered as affecting more than the mere naming of harmonies. The chord system teaches us that all harmonies are chords, are built by imposing major and minor thirds upon each other. The proposed system should, as will be shown, teach us to recognize harmonies which are built by superimposing any intervals. It should teach us a broader conception of harmonies and, as I believe, a more valuable one, since the importance of a notion usually depends upon its generality.

To-day the modern composer habitually employs the twelve-tone scale as the source of his harmonic invention. The abundance of accidentals in our modern composition is superficial but none the less accurate evidence of the passing of the feeling for the diatonic modes. To-day there are also a few scales which are formed of new arrangements in the intervals of our duodecimal system.

But the octave still remains the common basis for all scales now used; each scale repeats itself at successive octaves.

It seems only natural that we make this interval which is of the greatest importance because it has the simplest ratio, the basis for our harmony. We may therefore call its interval unity.[5]

Having established the octave as the basic interval, and having assigned to it the number one, we turn our attention to the lesser intervals. The semitone, since it is one-twelfth the gradation of the octave, will be known as the interval, one-twelfth. The "whole tone" becomes two-twelfths. Tabulating all of our intervals in their old and new nomenclature we have:

DIATONIC NAME	NATURAL OR CHROMATIC NAME
Minor Second	One Twelfth
Major Second	Two Twelfths
Minor Third	Three "
Major Third	Four "
Perfect Fourth	Five "
Augmented Fourth	Six "
Perfect Fifth	Seven "
Minor Sixth	Eight "
Major Sixth	Nine "
Minor Seventh	Ten "
Major Seventh	Eleven "
Octave	One.

Although many of these fractions expressing intervals are not reduced to their simplest form, it is of advantage to retain the common denominator, twelve; for if all intervals can be expressed as quotients of variable integers and the constant twelve, we need consider only the numerators and eliminate the common denominator. Thus the intervals, one-twelfth, two-twelfths, three-twelfths etc., may be called respectively, one, two, three, etc. It is clear that this nomenclature is founded entirely upon the chromatic scale since every interval is measured as a multiple of the intervals comprising the chromatic scale.

In naming harmonies having more than one interval, the advantages of the chromatic nomenclature are immediately apparent. For instance, the major triad is said to be formed of a minor third placed above a major third. In other words the interval 3 is placed above the interval 4. Thus the major triad in fundamental position is a $^3{}_4$ or 4-3 harmony. Likewise, the minor triad is a 3-4 harmony; the dominant sept chord in fundamental position, a 4-3-3 harmony; and the dominant sept chord in its first inversion is a 3-3-2 harmony. The harmony under previous discussion is, in the form

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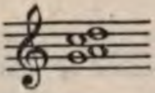
[5] For simplicity we call the interval unity, although the physical interval of the octave is 2.

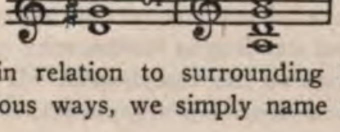


a 2-3-2 harmony.

This change in nomenclature means more than is at first apparent. It means an expansion of our conception of harmonies which may, perhaps, offset the limitations felt in the minds of many who can think of music only as varying arrangements of groups of superimposed thirds. We may freely think of any harmonies as being composed of superimposed intervals of any sort, instead of being shackled by considering every harmony made up of superimposed thirds, or inversions of these. Our vocabulary of harmonies, instead of embracing only chords, will embrace all harmonies.

It may be well here to forestall a possible objection that the chord system is the nearest to the ideal from the physical point of view. It is true that chords are physically the "cleanest" harmonies, i. e., their tones have the simplest vibration ratios. It may be said from this that the system of building up thirds is founded not upon an arbitrary choice, but upon an acoustic basis. But I answer that from this point of view it is immaterial whether we think of major and minor thirds or of 4s and 3s in building up the most important harmonies. If thirds and sums of thirds are the most important intervals, then, after we have learned the chromatic nomenclature, 3s, 4s, 6s and 7s will come to be recognized as the most important intervals. There is no ground for any charge that the chromatic nomenclature is more empiric and less scientific than the chord system.

Again, is not the chromatic nomenclature a simple and an accurate method for naming and classifying any harmony? Instead of grappling with such harmonies as these:



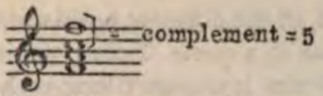
considering them in relation to surrounding harmonies, and in themselves by devious ways, we simply name them chromatically as consisting of superimposed intervals. They are respectively: 6-4-6-4 and 5-5-4-4 harmonies.

Our next task is to study more closely the nature of harmonies and to discover suitable means for systematically finding all of these harmonies.

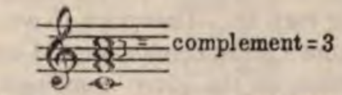
First we will recognize a tone essentially the same whether its vibration frequency is increased or diminished by a power of two; for the ordinary ears hear it as essentially the same. For this reason, we are compelled to regard an inversion of a harmony only as a different position of the harmony and not as a different harmony. In the chord system we decide upon one position of a harmony which we call close fundamenal, namely that position in which the harmony's root is in the base. (There is, however, no distinction made between "open" and "close" fundamental positions.) Likewise, and for purpose of classification, we should decide upon a fundamental position of a harmony in our chromatic system of nomenclature. The choice of a fundamental position, though arbitrary, will be made later in the discussion.

The relations between the tones comprising a harmony are intervals. A harmony may be thought of as a combination of intervals. However, a combination of intervals may form more than one harmony. The *major triad* is a 4-3 harmony, that is it is made up of the intervals 4 and 3; yet if we merely reverse the order of these intervals we have another harmony, the *minor triad*. It is clear then that a harmony is one permutation only of a given combination of intervals.

Now it might be thought that one could form all harmonies existing in our duodecimal system by permutating all possible combinations of intervals in all possible ways. However, by doing this we would calculate an immense number of harmonies, very many of which would only be inversions of each other. To avoid the repetitions occasioned by inversions, in classifying all harmonies, we come to the consideration of an extra interval with each harmony. If we invert the major triad or the 4-3 harmony we have first a 3-5 harmony (commonly called the first inversion) and then a 5-4 harmony (called the second inversion). The extra interval to be considered in this case is the interval 5, which is bounded by the highest tone of the triad and the note an octave above the triad's lowest tone, the triad standing in fundamental position—an interval which will be defined as the *complement*. If the triad is in 4-3 position of f, the complement is the interval c-f:



If the triad is in 5-3 position on c, the complement becomes the interval a-c or 3.



The complement will be denoted by the letter R in the following paragraphs. The term will be employed in the discussion of harmonies having any number of tones or intervals.

Now if A represents the lowest interval of a harmony in close position, that is, such a position in which all tones are within the compass of an octave; B the interval above A; C the interval above B, etc., and R the complement, a harmony could be represented thus:

R
.
.
.
. or ABC....R
C
B
A

The first inversion of this harmony would be represented thus:

A
R
.
. or BC..RA
.
C
B

the second inversion thus:

B
A
R
.
. or C...RAB[6]
.
.
C

---

[6] Each different letter does not necessarily denote a different sized interval.

We can think of all the different positions of the same harmony as arranged clockwise in a circle. Taking the intervals in clockwise order we find a different inversion taking each letter as a starting-point. It is clear that the inversions of a harmony are cyclic rotations of the permutation of intervals forming the fundamental position, whichever it may be. Therefore if we desire to form all possible harmonies from a given combination of intervals, we employ only those permutations of the given combination which are not cyclically related; that is, out of each group of permutations which are cyclic rotations of each other we select only one permutation as representative of a single harmony.

The Number of Harmonies with a Given Combination.

Assume we are given a harmony A-B, in which A and B are of different magnitude. Furthermore let us make A and B such intervals that the complement (R) of A-B, is unequal in magnitude to either A or B. Including R in our letter representation, we denote the harmony as A-B-R. The minor (3-4-5) and major (4-3-5) triads are good examples of such a harmony. We have already seen that the inversions of a harmony are cyclic rotations of its arrangement of intervals. Conversely, if two or more harmonies are cyclically related they are only different positions or inversions of one and the same harmony.

Since we are now engaged in finding how many harmonies can be formed from a given combination of intervals, our problem becomes one of finding the number of cyclically unrelated permutations of the given combination.

Let us experiment with our harmony A-B-R. The permutations of the combination are:

ABR	BRA	RAB
ARB	BAR	RBA

The arrangements in the upper row are cyclically related; likewise those in the lower; yet no one of the permutations in the upper row is related cyclically to any one in the lower row. Hence, only two harmonies: A-B-R and A-R-B, can be formed of the combination A, B, and R. We observe that both harmonies are represented in each vertical group; moreover in each vertical group one letter occupies the same position (i. e., first in this case) in both harmonies while the other two letters are permutated.

Let us experiment in like manner with a combination of 4 intervals (R included as one), in which all the intervals are of different magnitudes. We represent its parts as A, B, C, and R. Its permutations are:

ABCR	BCRA	CRAB	RABC
ABRC	BRCA	CABR	RCAB
ACBR	BRAC	CBRA	RACB
ACRB	BACR	CRBA	RBAC
ARBC	BCAR	CARB	RBCA
ARCB	BARC	CBAR	RCBA

Here, too, the permutations have been so arranged that those in any one horizontal row are cyclically related, while no permutation in one horizontal row is cyclically related to any one in any other horizontal row. Thus any one of the vertical columns contains all possible harmonies: in this case 3! or 6. It will be noticed here as before, that by retaining one letter in a stationary position throughout while permutating the remaining letters, all possible harmonies are formed from the given combination; for, although many of the permutations of the remaining letters are cyclically related, the stationary letter will bear a different relation to the other letters in each case. In each vertical column one letter is held in the same position, allowing the remaining 3 letters to be permutated in 3! different ways.

With a combination of two intervals (R included) we form 1! or 1 harmony; with three intervals (R included) we form 2! or 2, with four intervals we form 3! or 6, etc. Thus with n different intervals we form $(n-1)!$ different harmonies.

But this formula does not apply to combinations in which two or more intervals are alike; and such combinations are by far more numerous than the others. In fact, no harmony can have as many as five or more different intervals, provided of course that all its tones are bounded by the octave. To illustrate, we find the sum of the five smallest intervals (1, 2, 3, 4, 5) to be 15, which exceeds the octave 12 by 3.

To experiment with harmonies in which two or more intervals are alike let us take the combination: A, B, B, and R. Its permutations are:

ABBR	BBRA	BRAB	RABB
ABRB	BRBA	BABR	RBAB
ARBB	BARB	BBAR	RBBA

Here again the horizontal rows contain cyclically related permutations while the vertical columns do not. We find all three of the possible harmonies represented in any one vertical group (in each of which one letter has a stationary position throughout). However, since the combination contains two B's there are two "B rows," each of which contains the three harmonies. Thus if one B is held stationary while the remaining intervals are permutated, we obtain six instead of three harmonies; showing that our rule of stationaries[7] holds only for singly occurring intervals. But if we hold A stationary, we permutate B-B-R in $3!/2!$ different ways forming 3 different harmonies.

With the combination A, B, B, R and R we can form the following harmonies: ABBRR, ABRBR, ABRRB, ARBRB, ARBBR, ARRBB, by holding A in the same position and permutating the remaining letters in $4!/2!2!$ (=6) different ways.

The general formula then for the number of harmonies to a given combination in which at least one interval occurs singly is:

$$\text{H (harmonies)} = (n-1)!/n_1!\,n_2!\,n_3!\ldots\ldots$$

where n is the total number of intervals (R included) in the combination, and n_1, n_2, n_3, etc., are the respective number of times which certain intervals are found. This is the general formula for the number of harmonies to a combination, as the large majority of combinations contain singly occurring intervals. To find the number of harmonies in a combination containing no singly occurring intervals actual trial must be resorted to; any formula for this would be beyond the scope of this work. As an example of the error to which the formula leads if it is applied to combinations having no singly occurring interval, we will apply it to the combination 3 A's and 3 B's. Here

$$(n-1)!/n_1!\,n_2!\ldots. = 5!/3!\,3! = 1.2.3.4.5/(1.2.3)(1.2.3) = 10/3$$

The result is an impossibility, since a fractional number of harmonies cannot exist.[8]

The Number of Possible Combinations.

We will henceforth regard a harmony as in a *prime* position if its tones are reduced to within the compass of an octave.

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[7] That is, the method of holding one interval stationary and permutating the remaining intervals in all possible ways to obtain the several different harmonies.

[8] We remember that throughout the last pages we have considered the complement R as one of the n intervals of a combination. It might be supposed that we could now eliminate R from consideration and thereby make our formula, $n!/n_1!\,n_2!\ldots.$ This could not be done generally, since R might be an interval having a magnitude equal to that of another interval (i.e., A, B, or C, etc.) in the combination; in that case it would necessarily figure in the denominator of the fraction.

No harmonies in prime position can consist of less than two or more than twelve tones, in our duodecimal system. Hence no prime harmony can have less than two (R included) or more than twelve intervals. Now the number of combinations possible within the octave could be computed, but the result would be of no value, since in finding the number of harmonies we must treat each combination separately. Furthermore, mere numbers interest us only speculatively while a concrete method of obtaining all harmonies is of real value.

To simplify the task of finding the combinations possible with the twelve units within the octave, we will treat separately those groups of combinations having a different number of tones or intervals. A separate table will be made for each group of combinations having a certain fixed number of intervals. It is, of course, evident that the sum of the intervals of each combination equals the octave 12, since R is always one of the combination's intervals. In the tables the vertical columns contain the respective intervals, varying in magnitude as the number of intervals of the combination permit. In the horizontal rows are found the combinations. If a number greater than 1 is found in a combination, it indicates how many times the interval in whose vertical column it lies is repeated in the combination. Thus a number 3 lying in a column headed by the number 2, indicates that the combination contains the interval 2 (or the whole tone) thrice. The tables follow.[9]

TABLE I; N (NUMBER OF INTERVALS INCLUDING R)=2

	VARIOUS INTERVALS											CALCULATIONS OF HARMONIES	(NO. OF HARM.)
	1	2	3	4	5	6	7	8	9	10	11		H
1	1										1	$H=(n-1)!=(2-1)!$	1
2		1								1		"	1
3			1						1			"	1
4				1				1				"	1
5					1		1					"	1
6						2						"	1
												Total	6

---

[9] The combination numbers found to the left of the respective combinations are only for future reference.

TABLE III; N=4

	1	2	3	4	5	6	7	8	9	CALCULATIONS OF HARMONIES	H
1	3								1	H=3!/3!	1
2	2	1						1		H=3!/2!	3
3	2		1				1			"	3
4	2			1		1				"	3
5	2				2					By trial	2
6	1	2					1			3!/2!	3
7	1	1	1			1				3!	6
8	1	1		1	1					"	6
9	1		2		1					3!/2!	3
10	1		1	2						"	3
11		3				1				3!/3!	1
12		2	1		1					3!/2!	3
13		2		2						By trial	2
14		1	2	1						3!/2!	3

TABLE II; N=3

	1	2	3	4	5	6	7	8	9	10	CALCULATIONS OF HARMONIES	H
1	2									1	H=(3—1)!/2!	1
2	1	1							1		(3—1)!	2
3	1		1					1			"	2
4	1			1			1				"	2
5	1				1	1					"	2
6		2						1			2!/2!	1
7		1	1				1				2!	2
8		1		1		1					"	2
9		1			2						2!/2!	1
10			2			1					"	1
11			1	1	1						2!	2
12				3							By trial	1
											Total	19

TABLE IV; N=5

	1	2	3	4	5	6	7	8	CALCULATIONS OF HARMONIES	H
1	4							1	H=4!/4!	1
2	3	1					1		4!/3!	4
3	3		1			1			"	4
4	3			1	1				"	4
5	2	2				1			4!/2! 2!	6
6	2	1	1		1				4!/2!	12
7	2	1		2					4!/2! 2!	6
8	2		2	1					"	6
9	1	3			1				4!/3!	4
10	1	2	1	1					4!/2!	12
11	1	1	3						4!/3!	4
12		4		1					4!/4!	1
13		3	2						By trial	2
									Total	66

TABLE V; N=6

	1	2	3	4	5	6	7	CALCULATIONS OF HARMONIES	H
1	5						1	H=5!/5!	1
2	4	1				1		5!/4!	5
3	4		1		1			"	5
4	4			2				By trial	3
5	3	2			1			5!/2! 3!	10
6	3	1	1	1				5!/3!	20
7	3		3					By trial	4
8	2	3		1				5!/2! 3!	10
9	2	2	2					By trial	16
10	1	4	1					5!/4!	5
11		6						By trial	1
								Total	80

TABLE VI; N=7

	1	2	3	4	5	6	CALCULATIONS OF HARMONIES	H
1	6					1	$H=6!/6!$	1
2	5	1			1		$6!/5!$	6
3	5		1	1			"	6
4	4	2		1			$6!/2!\,4!$	15
5	4	1	2				"	15
6	3	3	1				$6!/3!\,3!$	20
7	2	5					By trial	3

Total 66

TABLE VII; N=8

	1	2	3	4	5	CALCULATIONS OF HARMONIES	H
1	7				1	$H=7!/7!$	1
2	6	1		1		$7!/6!$	7
3	6		2			By trial	4
4	5	2	1			$7!/2!\,5!$	21
5	4	4				By trial	10

Total 43

TABLE VIII; N=9

	1	2	3	4	CALCULATIONS OF HARMONIES	H
1	8			1	$H=8!/8!$	1
2	7	1	1		$8!/7!$	8
3	6	3			By trial	10

Total 19

TABLE IX; N=10

	1	2	3	CALCULATIONS OF HARMONIES	H
1	9		1	$H=9!/9!$	1
2	8	2		By trial	5

Total 6

TABLE X; N=11

	1	2	CALCULATIONS OF HARMONIES	H
1	10	1	H=10!/10!	1

TABLE XI; N=12

	1	CALCULATIONS OF HARMONIES	H
1	12	By trial	1

Let us tabulate the number of harmonies found in each respective table:

Table No.	1	Intervals	2	Total No. H	6
" "	2	"	3	" " "	19
" "	3	"	4	" " "	43
" "	4	"	5	" " "	66
" "	5	"	6	" " "	80
" "	6	"	7	" " "	66
" "	7	"	8	" " "	43
" "	8	"	9	" " "	19
" "	9	"	10	" " "	6
" "	10	"	11	" " "	1
" "	11	"	12	" " "	1
				Total	350

We notice that as the number of intervals increases to 6 the number of harmonies increases, while as the number of intervals increases beyond 6 the number of harmonies decreases. Thus, six-tone harmonies (or harmonies of six intervals including R) are most numerous; five- and seven-tone harmonies next in number; four- and eight-tone harmonies next; etc. More harmonies can be formed from combination 4 Table VII than from any other combination. From it we obtain 21 harmonies. Finally we see that the total number of harmonies in the duodecimal system is 350.

The harmonies of least dissonance will be those having the fewest small intervals. There are 9 harmonies having intervals no smaller than 3 (minor third); there are 28 having intervals no smaller than 2 (major second); and there are 55 harmonies having only one interval, 1 (minor second).

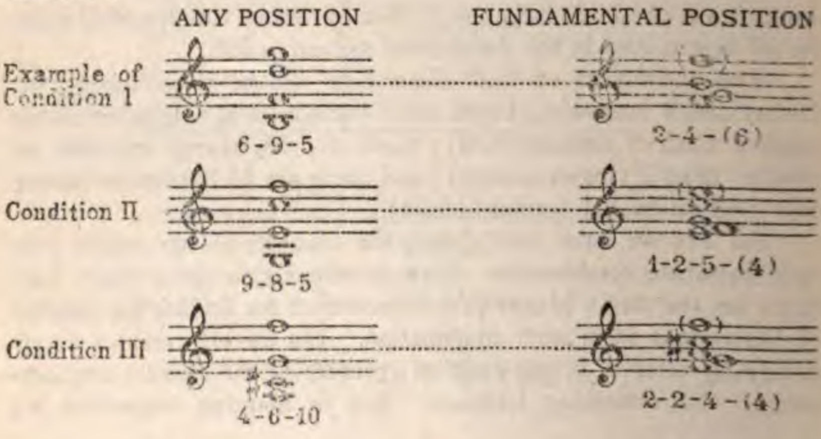
So far we have only shown the number of harmonies with each separate combination. Now it remains to show every harmony on the staff. Means have been shown for finding the number of harmonies from each combination. We merely retain a singly occurring interval in one position (preferably the lowest) and permutate the remaining intervals. But in notating harmonies we should at least represent them in some standard form; and thus we arrive at the long-delayed decision about fundamental position.

Fundamental Position.

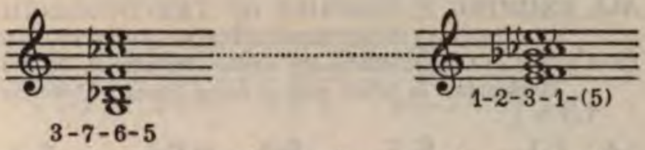
Among the 350 complex harmonies which we have found, there are many—nay, a large majority—which could not be represented as plain chords. Furthermore, since our vocabulary of intervals and harmonies has become a chromatic one, we will no longer attempt to reconcile the limited number of harmonies known as chords, with all of 350 harmonies; hence no attempt to make a chordic position the fundamental form. Arbitrarily we might choose as our fundamental a form in which the *span,* or the interval between the extreme tones of a harmony, is smallest. Or, since we have found it convenient to place any singly occurring interval in the lowest position in forming harmonies from combinations, we might call such a position fundamental. The question is difficult, and although my solution is only arbitrary I believe the fundamental position should be one which satisfies the following conditions:

I. The harmony is prime.
II. The smallest interval (it may be R) occupies the lowest position, and thereby becomes A.
III. In case there exist two or more smallest intervals, the one or more other smallest intervals are nearest A.
IV. In case the two or more smallest intervals are spaced regularly apart, the next smallest interval is nearest A.

Illustrations follow in order respective to the conditions of the definition.



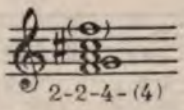
Condition IV



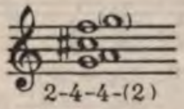
The first example illustrates how a harmony in more or less spread-out position (left-hand column) is reduced to prime position.

The second illustrates how another harmony (9-8-5) is reduced to prime position, following which it is placed so that the smallest interval (1) occupies the lowest position.

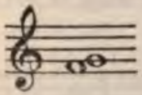
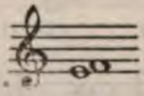
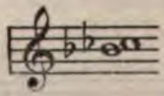
The third illustrates a harmony which, in any prime position, contains two smallest intervals. We are not satisfied with making either of these A; the equivalent of A must be nearest A. Thus in this position:



A's equivalent is nearer A than in the position:



The fourth illustrates a harmony containing two smallest intervals (=1) which are equally separated in any prime position of the harmony. Thus the two intervals (1) are always separated by the interval 5. But because the next smallest interval

 lies nearer  than 

we consider the interval e-f as A.

Thus in classifying any harmony, only three short steps are necessary. First we reduce its tones to within the compass of the octave; second, we select from the prime positions the fundamental position; third, we name the harmony according to the chromatic nomenclature.

Having determined the fundamental position, we are prepared to write out on the staff all existing harmonies with the help of the previous tables. Every harmony will appear in fundamental form; while each will be respectively named. The harmonies follow:

ALL EXISTING HARMONIES OF THE DUODECIMAL SY

LISTED IN FUNDAMENTAL POSITIONS AND BY TABLES.

(The names of the harmonies are written respectively below; the nu
combination in which each is found appears above the staff.)

Table I

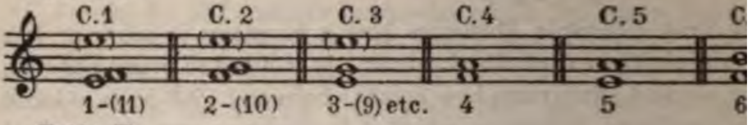


Table II

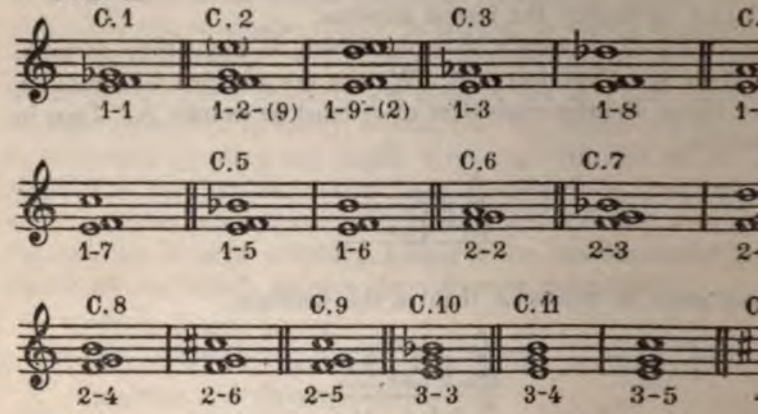
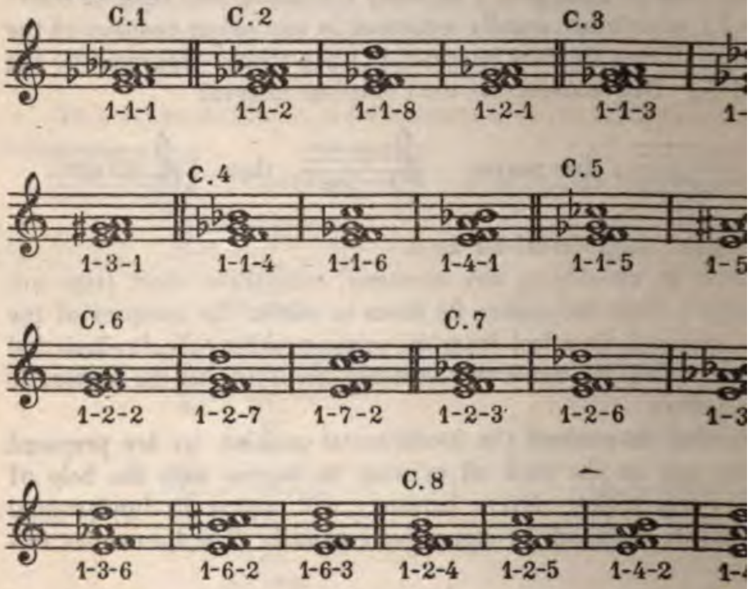


Table III



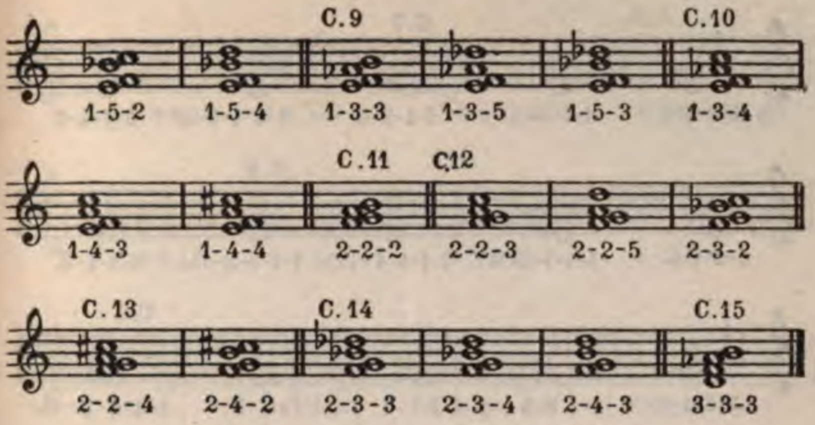
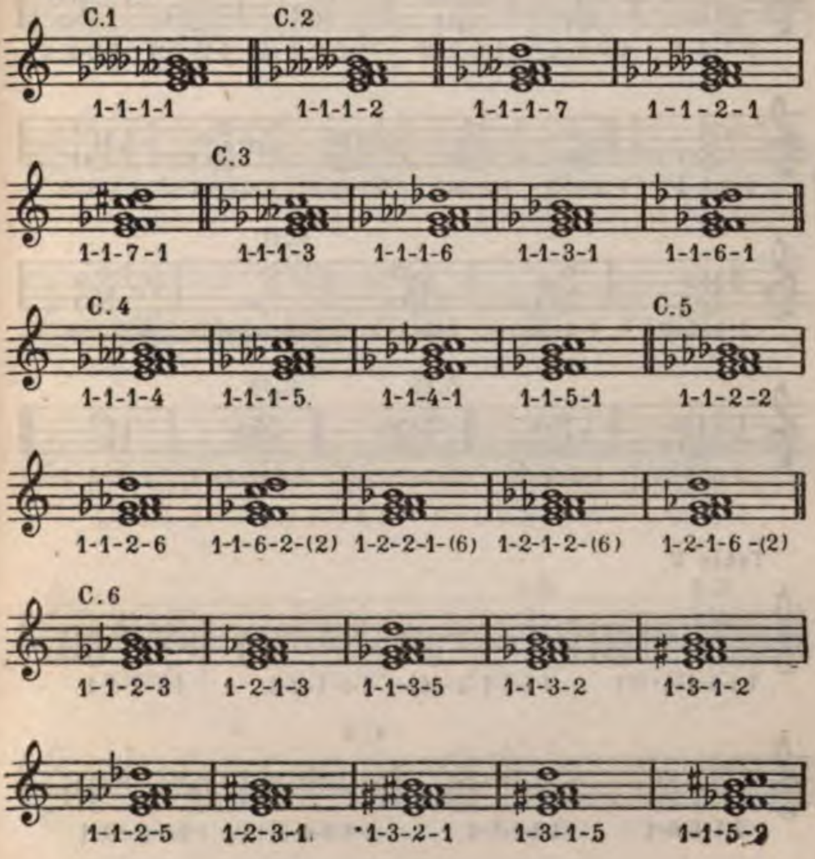

C.9
C.10
1-5-2
1-5-4
1-3-3
1-3-5
1-5-3
1-3-4
C.11
C.12
1-4-3
1-4-4
2-2-2
2-2-3
2-2-5
2-3-2
C.13
C.14
C.15
2-2-4
2-4-2
2-3-3
2-3-4
2-4-3
3-3-3

Table IV


C.1
C.2
1-1-1-1
1-1-1-2
1-1-1-7
1-1-2-1
C.3
1-1-7-1
1-1-1-3
1-1-1-6
1-1-3-1
1-1-6-1
C.4
C.5
1-1-1-4
1-1-1-5.
1-1-4-1
1-1-5-1
1-1-2-2
1-1-2-6
1-1-6-2-(2)
1-2-2-1-(6)
1-2-1-2-(6)
1-2-1-6-(2)
C.6
1-1-2-3
1-2-1-3
1-1-3-5
1-1-3-2
1-3-1-2
1-1-2-5
1-2-3-1
1-3-2-1
1-3-1-5
1-1-5-2

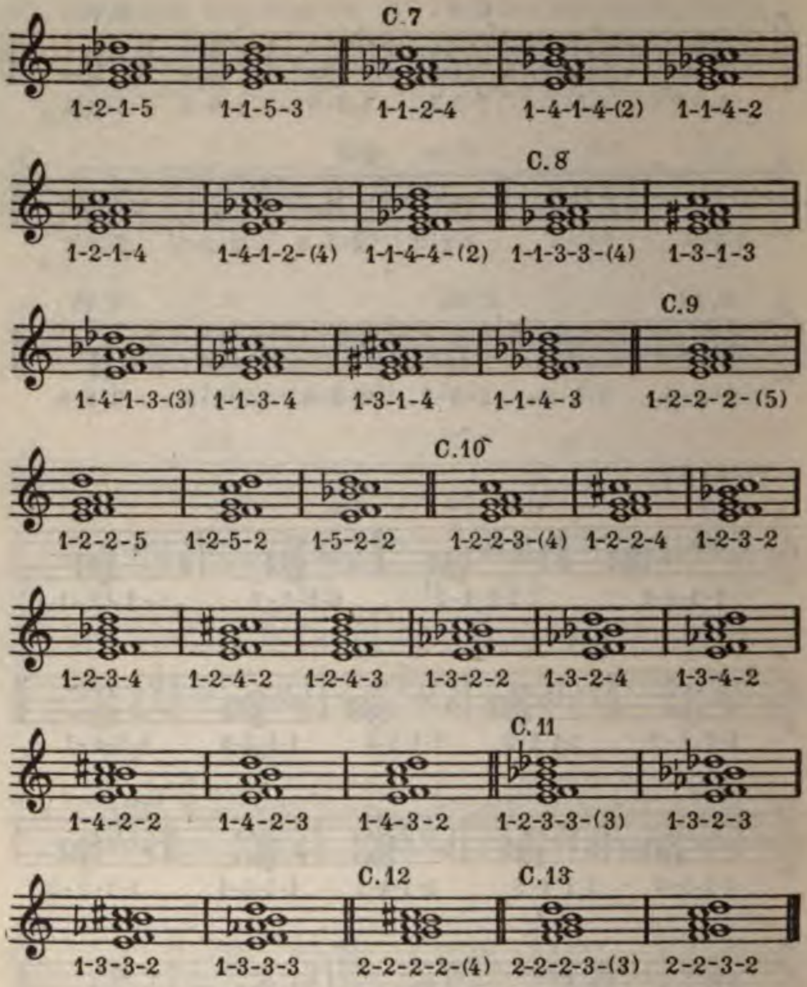
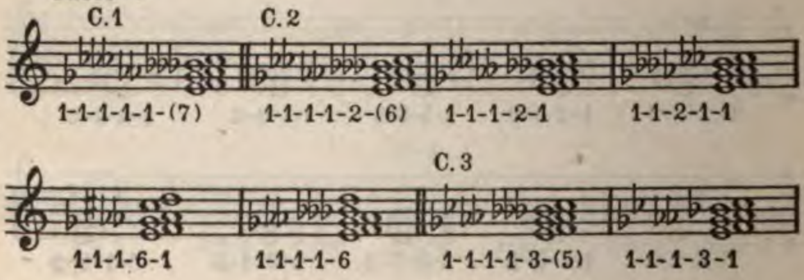
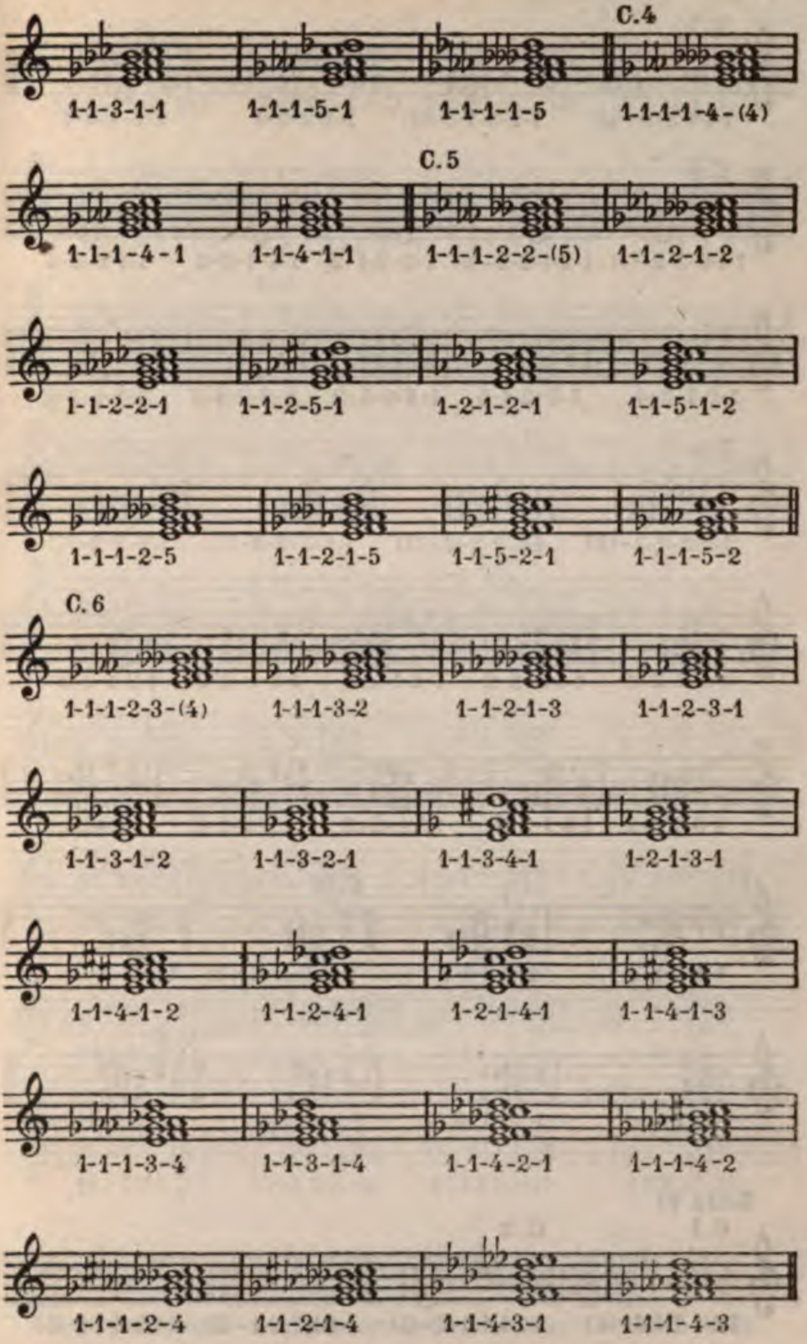

C.7
1-2-1-5
1-1-5-3
1-1-2-4
1-4-1-4-(2)
1-1-4-2
C.8
1-2-1-4
1-4-1-2-(4)
1-1-4-4-(2)
1-1-3-3-(4)
1-3-1-3
C.9
1-4-1-3-(3)
1-1-3-4
1-3-1-4
1-1-4-3
1-2-2-2-(5)
C.10
1-2-2-5
1-2-5-2
1-5-2-2
1-2-2-3-(4)
1-2-2-4
1-2-3-2
1-2-3-4
1-2-4-2
1-2-4-3
1-3-2-2
1-3-2-4
1-3-4-2
C.11
1-4-2-2
1-4-2-3
1-4-3-2
1-2-3-3-(3)
1-3-2-3
C.12
C.13
1-3-3-2
1-3-3-3
2-2-2-2-(4)
2-2-2-3-(3)
2-2-3-2

Table V


C.1
C.2
1-1-1-1-1-(7)
1-1-1-1-2-(6)
1-1-1-2-1
1-1-2-1-1
C.3
1-1-1-6-1
1-1-1-1-6
1-1-1-1-3-(5)
1-1-1-3-1


C.4
1-1-3-1-1
1-1-1-5-1
1-1-1-1-5
1-1-1-1-4-(4)
C.5
1-1-1-4-1
1-1-4-1-1
1-1-1-2-2-(5)
1-1-2-1-2
1-1-2-2-1
1-1-2-5-1
1-2-1-2-1
1-1-5-1-2
1-1-1-2-5
1-1-2-1-5
1-1-5-2-1
1-1-1-5-2
C.6
1-1-1-2-3-(4)
1-1-1-3-2
1-1-2-1-3
1-1-2-3-1
1-1-3-1-2
1-1-3-2-1
1-1-3-4-1
1-2-1-3-1
1-1-4-1-2
1-1-2-4-1
1-2-1-4-1
1-1-4-1-3
1-1-1-3-4
1-1-3-1-4
1-1-4-2-1
1-1-1-4-2
1-1-1-2-4
1-1-2-1-4
1-1-4-3-1
1-1-1-4-3

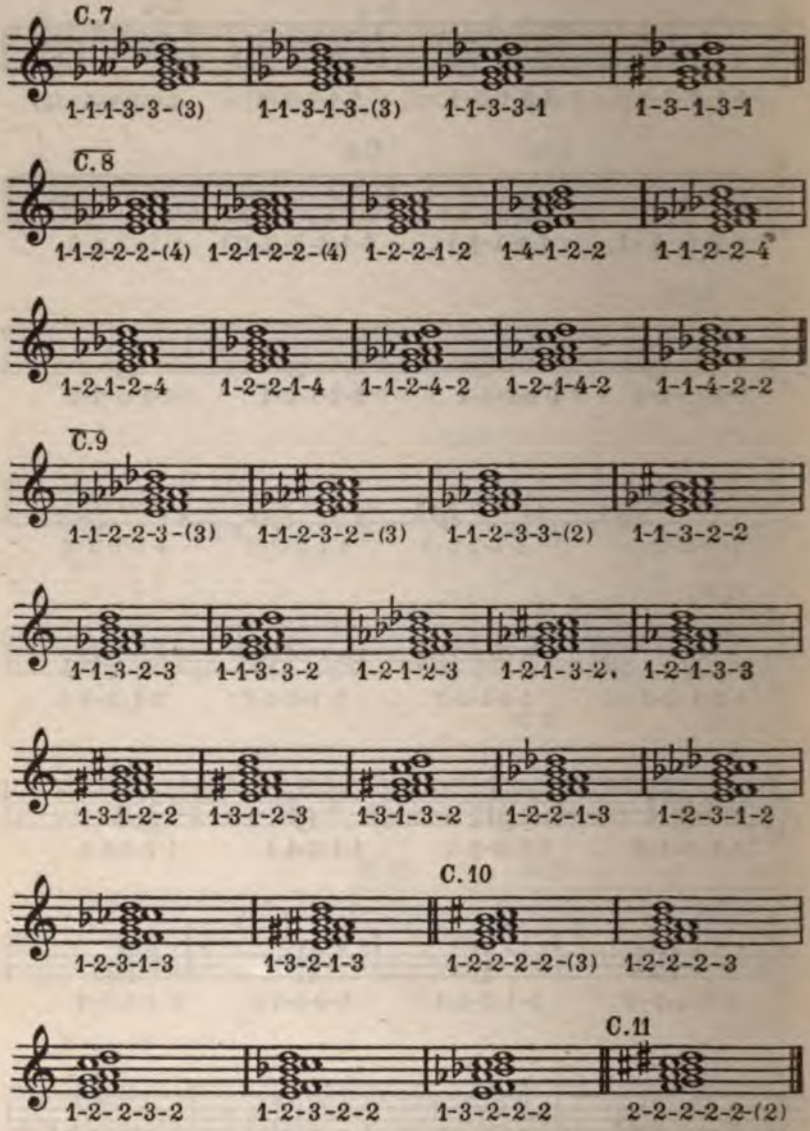
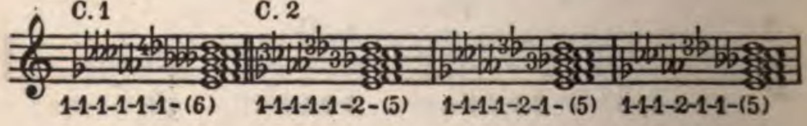
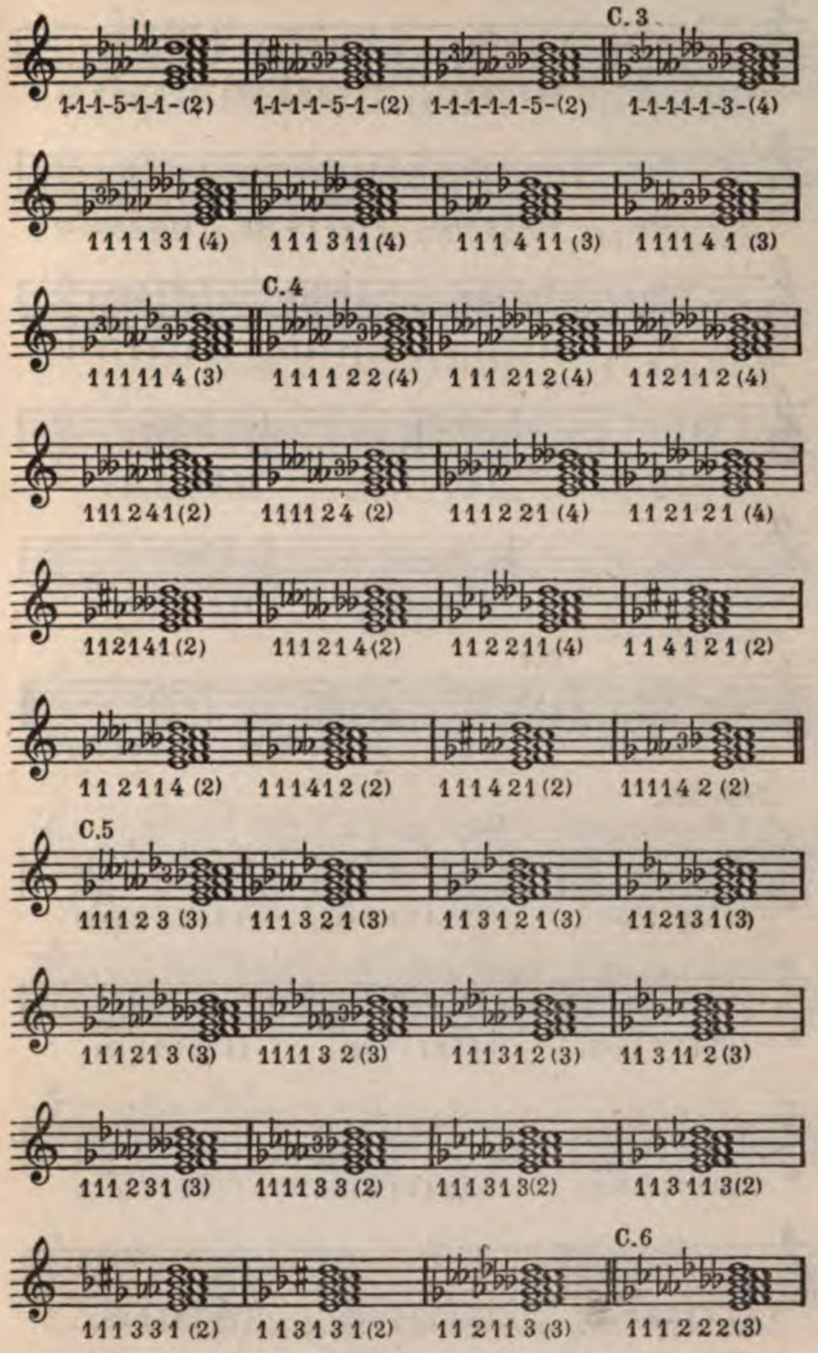

C.7
1-1-1-3-3-(3)
1-1-3-1-3-(3)
1-1-3-3-1
1-3-1-3-1
C.8
1-1-2-2-2-(4)
1-2-1-2-2-(4)
1-2-2-1-2
1-4-1-2-2
1-1-2-2-4
1-2-1-2-4
1-2-2-1-4
1-1-2-4-2
1-2-1-4-2
1-1-4-2-2
C.9
1-1-2-2-3-(3)
1-1-2-3-2-(3)
1-1-2-3-3-(2)
1-1-3-2-2
1-1-3-2-3
1-1-3-3-2
1-2-1-2-3
1-2-1-3-2
1-2-1-3-3
1-3-1-2-2
1-3-1-2-3
1-3-1-3-2
1-2-2-1-3
1-2-3-1-2
1-2-3-1-3
1-3-2-1-3
C.10
1-2-2-2-2-(3)
1-2-2-2-3
1-2-2-3-2
1-2-3-2-2
1-3-2-2-2
C.11
2-2-2-2-2-(2)

Table VI


C.1
C.2
1-1-1-1-1-1-(6)
1-1-1-1-1-2-(5)
1-1-1-1-2-1-(5)
1-1-1-2-1-1-(5)


C. 3
1-1-1-5-1-1-(2)
1-1-1-1-5-1-(2)
1-1-1-1-1-5-(2)
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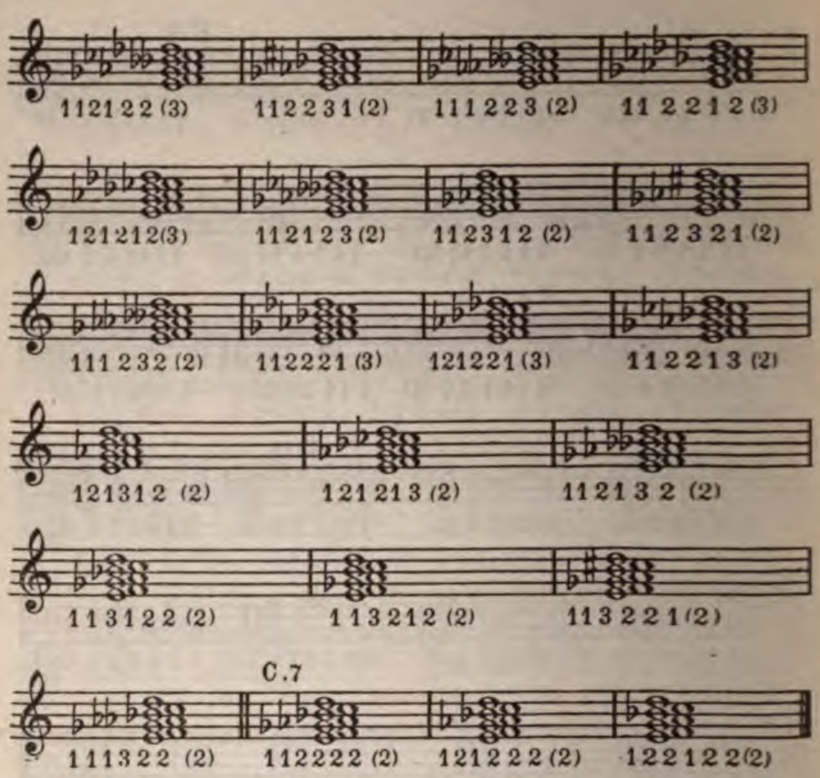
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112132 (2)
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C.7
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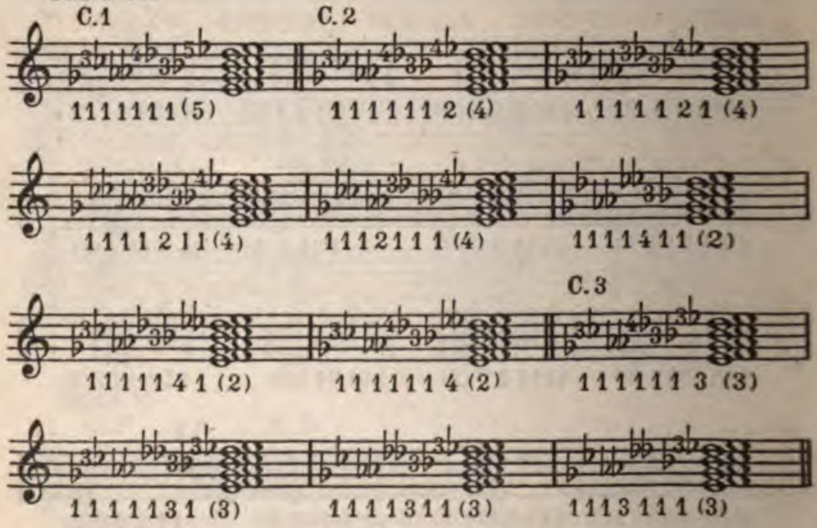
Table VII


C.1
C.2
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C.3
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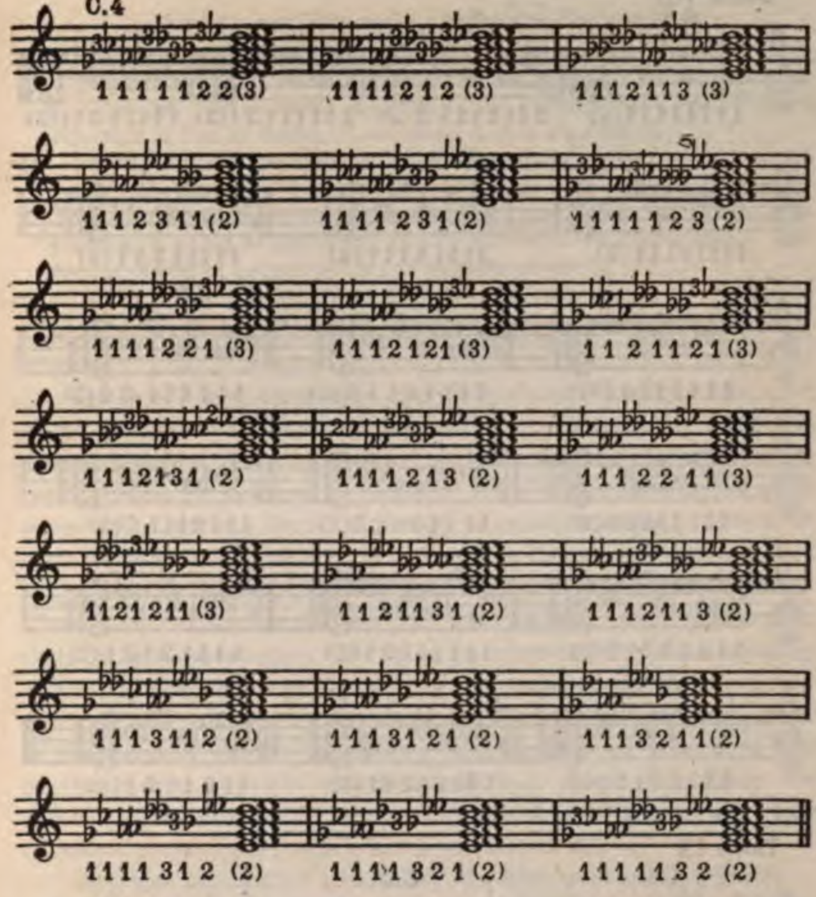
1113111 (3)

C.4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C.5

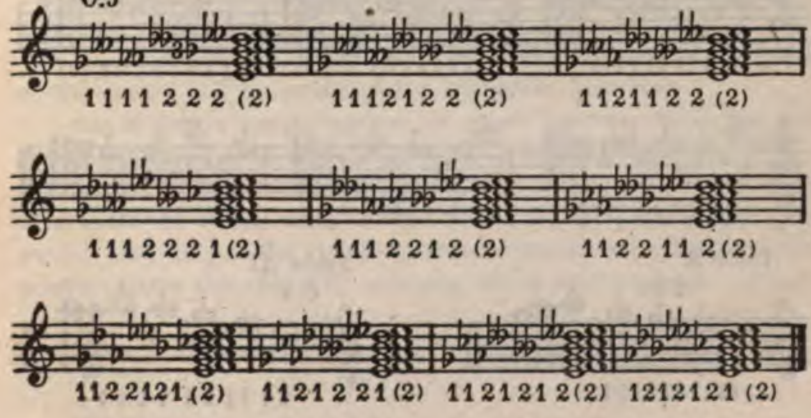
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Table VIII

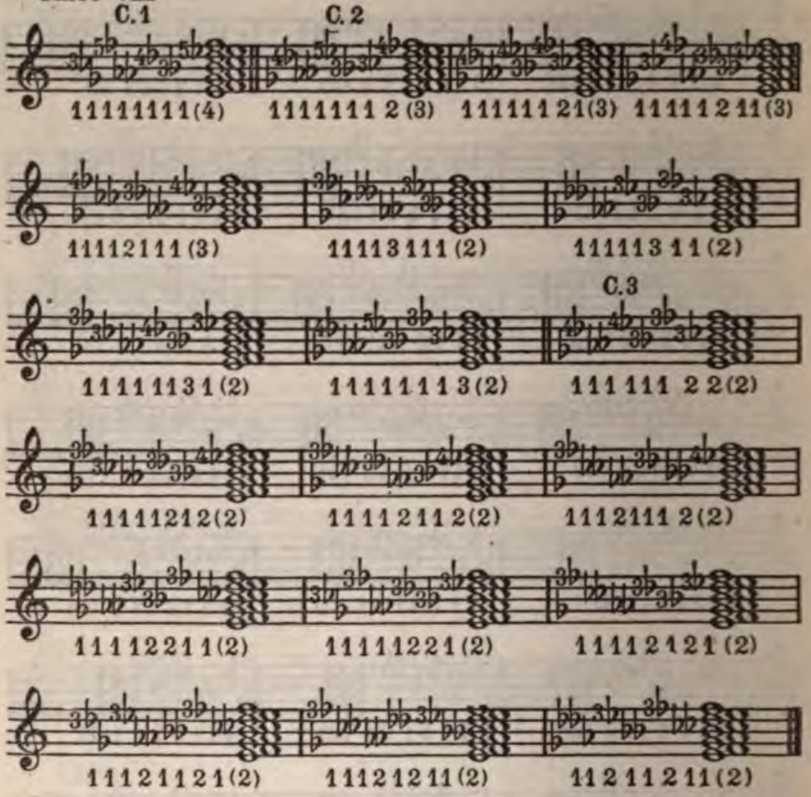


Table IX

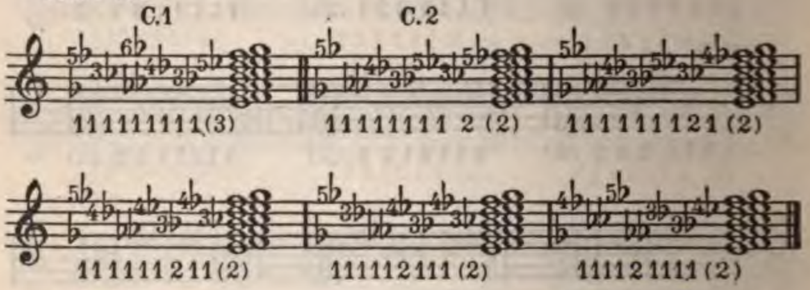


Table X

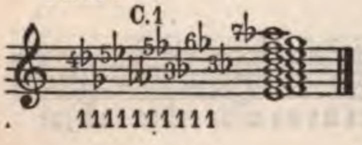
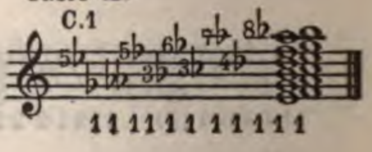


Table XI



Notes on the Notation of the Above Harmonies.

1. R is represented in parentheses where it is included in the notation.
2. The degree most convenient for representation is chosen for the base note.
3. The Number Names under each respective harmony have their integers separated at first by dashes. Later these dashes are omitted.
4. When the number of sharps and flats becomes excessively great, it is written *n* ♭ or *n* ♯. Thus ♯♯♯♯ becomes 4♯.
5. There are other means of notating some of these denser harmonies. For example, it would be possible to employ two staffs, or double stems. Our present system of notation allows of no better methods.

Inversions of the Harmonies.

Many of these harmonies, especially those of many tones, may sound unesthetic in their fundamental form because of their dissonance, even to an ear trained to an appreciation of the most "ultra-modern" music. A conglomeration of slow beats caused by adjacent tones will, indeed, almost approach a common noise. However, such dissonance can be largely reduced in the same harmony when the tones of the fundamental position are scattered by octaves. Thus many harmonies, seemingly obscure in their fundamental position, become more appreciable to us by inverting them or spreading them out. The different forms and inversions of almost every harmony (made possible by the range of modern instruments) allow of the greatest variety of effects. The number of inversions and positions of most harmonies is astounding. Now, in making our rather superficial study of inversions, we will be obliged to use a few technical expressions; which are enumerated below.

A prime position of a harmony has been previously defined.

Any prime inversion of a fundamental[10] harmony will be known as *primary* inversion.

An inversion not prime, but containing no interval as great as the octave will be considered a *secondary* inversion.

An inversion containing one or more intervals exceeding the octave in magnitude will be known as a *tertiary* inversion.

An example of each type is given, respectively, below; the three inversions are in the same harmony. Although a form like (3) would, according to the chord system, be considered a fundamental position since the root (e) occupies the lowest position, we will find it more convenient to regard any position which is not prime, even though it have the root in lowest position, as an inversion.

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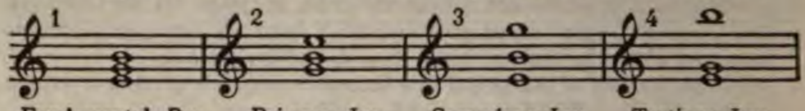
[10] That is, a harmony in fundaemntal position, according to definition.



Fundamental Pos. Primary Inv. Secondary Inv. Tertiary Inv.

In the following paragraphs, a few general principles are discussed in the form of propositions.

Proposition I. A harmony of n tones has $(n-1)$ inversions of the first degree.

Since a prime inversion can be formed with each tone as a lowest tone, and since $(n-1)$ tones are available as lowest tones (one tone being employed as the lowest tone for the fundamental) it follows that there are $(n-1)$ inversions possible.

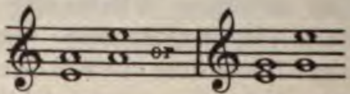
In other words, an n-tone harmony has n prime positions.

Proposition II. The number of secondary inversions of a harmony of n tones is $(n!-n)$.

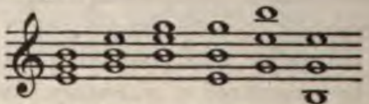
Let us experiment with two-, three- and four-tone harmonies, as follows, allowing no interval between adjacent notes to be as great as 12.

With two-tone harmonies we can form only two or 2! positions which conform to our limitations.

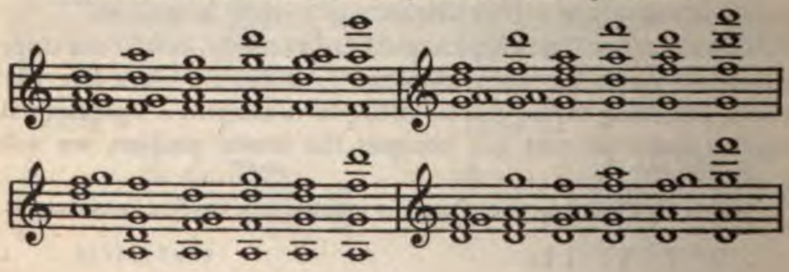
For example:



With three-tone harmonies only 6 or 3! such positions are possible; as for example:



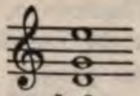
With four-tone harmonies only 24 or 4! are possible:



With 5-tone harmonies 5! or 120 such inversions are possible, etc., etc. With n tones $n!$ positions of this type are possible. But since these positions in which the intervals are less than 12 include n prime positions, the secondary inversions number $(n!-n)$.

It is evident that the number of tertiary inversions is entirely dependent upon the range of the instrument employed to represent them.

By the *span* of a harmony is meant the interval of its extreme tones or the sum of its intervals. Thus the span of



5-9

is $(9+5)$ equals 14.[11]

Proposition III. In a harmony of n tones the sum of the spans of its prime positions is $(n-1)$ octaves. Let us take an example from a 4-tone harmony. Adding its prime positions we have

$$\begin{array}{l}
A + B + C \\
\phantom{A +{}} B + C + R \\
A \phantom{{}+ B} + C + R \\
A + B \phantom{{}+ C} + R \\
\hline
3A+3B+3C+3R=3(A+B+C+R)=3 \text{ octaves} \\
\qquad\qquad\qquad\qquad\qquad\qquad\quad =(4-1) \text{ octaves.}
\end{array}$$

Progressions of Harmonies.

This subject interests us more from a speculative than a practical standpoint, since the possibilities in this direction are well-nigh unlimited, as will be shown. However, if the few suggestions that follow are carried out in limited form practical ends are attainable.

Any harmony may of course be preceded or followed by any other harmonies. Whether the progressions between harmonies sound abrupt or smooth depends partly upon the harmonies in question, partly upon the positions chosen, and partly upon the degree of broad appreciation to which we have been trained. However, smoothness or abruptness of progression does not concern us here, for either may be more desirable according to the character of a composition. The inclination of a composer with ideas certainly deserves more consideration than the ever weakening law of theorists.

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[11] In finding the span of a harmony, R is evidently not included to make the sum 12, since the span of all harmonies would consequently be multiples of 12.

Let us now calculate the number of progressions of two successive harmonies which can be made with a given number of harmonies, let us say n harmonies. One of these n harmonies placed upon one degree of the chromatic scale can progress to the same harmony placed on 11 other degrees. The same harmony can form 12 progressions with any of the other harmonies given, since any other harmony can be placed on 12 different degrees. And since there are $(n-1)$ such other harmonies, the first harmony can form $(n-1)12$ progressions with the remaining harmonies. Thus the total number of progressions possible between a single harmony and the remaining harmonies is: $11+12\cdot(n-1)$.

But as many progressions are possible with each of the $(n-1)$ remaining harmonies. Hence the total number of progressions of two successive harmonies possible with n harmonies is:

$$n[11+12(n-1)]=(12n-1)n \text{ or } 12n^2-n.$$

Thus, with only 2 harmonies we can form $(24-1)2$, or 46 progressions. With 5 harmonies (which is the limit of vocabulary with many persons, and in which may be included the major triad, minor triad, dominant sept, supertonic sept and leading-tone sept), $(60-1)5$ or 295 progressions of only two successive ones are possible. With the 350 existing harmonies, the possibilities of progression of two at a time are: $(350\times 12-1)\,350=1{,}469{,}650$.

The number of possibilities of progressions of three at a time will be $(12n^2-n)12n$, since each progression of two harmonies may be followed by one of n harmonies placed on any one of 12 different degrees of the scale. Thus with 350 harmonies 6,172,530,000 progressions of three are possible.

The general formula expressing the number of progressions possible is:

$$(12n^2-n)(12n)^{s-2}$$

where n is the number of harmonies among which the progressions are to be made, while s is the number of harmonies to be used at a time in a progression.

As mentioned before, the enormous figures just given mean little practically, yet they serve to emphasize the fact that variety is not only desirable but possible; and this is only variety of one kind, harmonic variety.

Melody, rhythm and form are quite as variable as harmony, and the variability of music is measured as the product of these respective elements of it and is therefore quite beyond the bounds of comprehension. Formerly I shared the foolish and common fear that as more music is written the possibilities of future invention narrow. I actually felt that the field for contemporary composition is narrower than it was a century ago. To-day it seems to me that every great musical work enlarges the field of the future.

"And myriad strains are there since the beginning still waiting for manifestation."[12]

ERNST LECHER BACON.

CHICAGO, ILLINOIS.

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[12] Busoni, *A New Esthetic of Music.*

CRITICISMS AND DISCUSSIONS.

THE PRIMITIVE AND THE MODERN CONCEPTIONS OF PERSONAL IMMORTALITY.

In an interesting review of my recent book (*The Beliefs in God and Immortality*: *an Anthropological, Psychological, and Statistical Study*) in the April number of this journal, it is inadvertently written that the book is "simply a statistical investigation." This statement is true of Part II only. It is not applicable to Part I, for it treats exclusively of "The two conceptions of immortality: their origins, their different characteristics, and the attempted demonstration of the truth of the modern conception." No more does the statement apply to Part III, which discusses "The present utility of the beliefs in personal immortality and in a personal God."

In the first half of the present paper, I set forth very briefly that which I consider the main contribution contained in Part I of my book. In the second half, I give some information concerning the statistics (Part II). J. H. L.

A curious contradiction seems to exist with regard to the origin of the belief in survival after death. It is authoritatively affirmed by anthropologists that that belief is to be found in every tribe now extant. Frazer, less dogmatic, writes that "it might be hard to point to a single tribe of men, however savage, of whom one could say with certainty that the faith is totally wanting among them."[1] And yet historians no less competent in their field than the anthropologists to whom we refer state with disconcerting unanimity that the belief in immortality appeared late among the people from whom Europe drew its civilization. We are told, for instance, that the Israelites' belief in immortality cannot be traced much further back than the beginning of the Christian era. The covenant Yahveh made with his people does not allude to a future life. The nation alone was an object of his care. The great prophets themselves, when they inveigh against sin, care only for the danger therefrom to the existence of the nation. Among the Greeks also the belief in immortality is said to have appeared late. Pythagoras, the Mysteries, and Plato are named as marking the rise of the faith. Of the Romans, Carter says that they did not have the idea of a personal soul: "It was not present at the time of the Punic wars. We see only scanty traces of it in the literature of the Ciceronean age."[2]

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[1] J. G. Frazer, *The Belief in Immortality*, pp. 25, 33.

These apparently contradictory affirmations may be explained in two ways: either the particular survival-idea expressed in the belief in ghosts, universal among primitive people, had at the beginning of the historical period disappeared from among the nations just mentioned; or the immortality which the historians of these nations have in mind is so different from the primary conception of continuation after death that they disregard that belief.

The first of these two suppositions is not tenable. When the historical period opens, a belief in survival was incontrovertibly present among the peoples of whom the historians we have quoted speak. In the Old Testament traces of polydemonistic belief are definite enough to preclude divergence of opinion. The evidence is just as clear in the case of the Greeks and of the Jews. The Homeric conception of man is of a dual personality composed of a visible earthly being and of its shadow or copy, which manifests its presence in dreams and continues to live in Hades after the severance of death. Jane Harrison has conclusively demonstrated that while the religion of the Olympic gods was in process of formation, and even much later, the Greeks practised rites clearly indicative of the belief in human ghosts.[3]

The idea of *manes*, essential to the religion of the old Romans, is a "vague conception of shades of the dead dwelling below the earth."[4] If one is to believe Lucretius, and there seems to be no reason why he should not be credited in this particular, the Romans were haunted by a dread of the judgment to come.

If the presence at the beginning of the historical period of practices indicative of a belief in survival, in the very people among whom the idea of immortality is said to have appeared late, is no longer a moot point, shall we hold that the kind of continuation after death which our historians have in mind when they deny the existence of the belief in immortality at the beginning of the historical period is so different from the idea entertained by the savage that they do not take that belief into account? The present paper will show that the early conception of survival after death—let it be called the primitive conception—is, as a matter of fact, radically different from the modern conception in point of origin, of nature, and of function.

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[2] J. B. Carter, *The Religious Life in Ancient Rome*, p. 72.

[3] Jane Harrison, *Prolegomena to the Study of Greek Religion*, 1st ed., p. 11.

[4] W. Ward Fowler, *The Religious Experience of the Roman People*, p. 386.

What was the nature of the primitive belief in the countries bordering the eastern end of the Mediterranean Sea at the period to which it is customary to trace the rise of the belief in immortality? Let us begin with Egypt, the land of the "religion of eternal life." The oldest historical documents we possess, the inscriptions in the passages and chambers of the great pyramids, called the Pyramid texts, belong to an already complex civilization although they date bask to about 3400 B. C. The glimpses of earlier belief found in these texts suffice, however, to indicate the presence of a religion of the underworld according to which the dead continued in unhappy existence under the earth. "The prehistoric Osiris faith," writes Breasted, "involved a forbidding hereafter which was dreaded." In an inscription on a stela addressed by a dead wife to her husband we read: "Oh my comrade, my husband. Cease not to eat and drink, to be drunken, to enjoy the love of women, to hold festivals. Follow thy longing by day and by night. Give care no room in thy heart. For the West Land (a domain of the dead) is a land of sleep and darkness, a dwelling-place wherein those who are there remain."[5]

The Babylonian dead were supposed to dwell in a great cave underneath the earth, the most common name of which was Arula. It "was pictured as a vast place, dark and gloomy....surrounded by seven walls and strongly guarded, it was a place to which no living person could go and from which no mortal could ever depart after once entering it."[6] For the Babylonians death made all men equal. There were no distinctions of rank in the underworld; kings, priests, conjurers, magicians, and common people, all found themselves together in the dry and dusty *kurnugea*. Everything one touched was dusty. Dust and earth were the food, the muddy water the drink of those living the shadowy life of the underworld.[7] They lived an ineffective, drowsy, starved existence.

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[5] A. Wiedemann, *The Realms of the Egyptian Dead*, p. 28.

[6] Morris Jastrow, *Aspects of Religious Belief and Practice in Babylonia and Assyria*, pp. 353, 356, 358.

Sheol of the Hebrews, like the underworld of the Babylonians, was a place of dread. The shades were forgotten of God. Yahveh was the God of the living, not of the dead. "Go thy way," says Ecclesiastes, "eat thy bread with joy, and drink thy wine with a merry heart....Let thy garments be always white; and let not thy head lack oil. Live joyfully with the wife thou lovest all the days of thy life of vanity....for there is no work, nor device, nor knowledge, nor wisdom, in Sheol whither thou goest."

In Greece also the souls went to the land of the dead bemoaning their lot, for it was wretched. From that dark country souls never returned. Homer draws a repulsive picture of the dead hovering in the dark realm of Acheron, hazily conscious, hollow voiced, weak, and indifferent.

Neither the Egyptians, nor the Babylonians, nor the Hebrews, nor the Greeks could, it seems, think of beings deprived of a vigorous, effective body as enjoying a happy life. The few fortunate individuals who were translated to Elysium or elsewhere without passing through death and lived on happily, had retained their body. The knowledge of the decomposition of the body after death and of the tenuous unsubstantial nature of ghostly apparitions, account naturally enough for the weakness and ineffectiveness attributed to ghosts.

For centuries this repulsive and hopeless belief oppressed the millions from among whom was to rise European civilization. A turning point had, however, been reached at the dawn of the historical period. The primitive belief was apparently doomed, for the leaders in those nations had not only felt the social danger it threatened, and had in consequence begun to deprecate as evil the cult addressed to ghosts, but they had also become clearly conscious of moral cravings, the satisfaction of which death seemed to make impossible.

Regarding the opposition that had arisen to the primitive belief, we may recall that in Israel, the religion of Yahveh was the determined enemy of the cult of the dead in all its forms. And of the Greeks we are told by Jane Harrison that "that which was in the sixth and even in the fifth century before the Christian era the real religion of the main bulk of the (Hellenic) people, a religion not of cheerful tendance but of fear and deprecation," was the same that Plutarch centuries later, and with him most of his great contemporaries, regarded as superstition. Among the Romans, ghosts had so far lost individuality as to be regarded by modern historians as impersonal forces. The cult had become to an amazing degree a matter of mere conventional behavior.[8] Thus a period of greatly decreased influence among the people of the primitive belief in immortality and of definite antagonism to it by the leaders had arrived.

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[7] Friedrich Delitzsch, *Das Land ohne Heimkehr, die Gedanken der Babylonier-Assyrer über Tod und Jenseits*, p. 16. He thinks, however, that as early as the thirtieth century B. C. a distinction in the abode of the shades made its appearance. Some of them lived in peace and comfort in a country provided with water (pp. 18-22).

Simultaneously with this opposition to the old belief, the consciousness of the insufficiency of this life to satisfy the cravings of the heart and the demands of conscience manifested itself in various and increasingly significant ways. One notes precursory signs: for instance, the averred translation of Menelaus to Elysium; of Ganymede to Olympus; of Parnapishtim to an earthly paradise somewhere in Mesopotamia; of Enoch, who was taken up unto his Lord; and of Elijah, who was carried in a chariot of fire by a whirlwind into Heaven. One notes also the appearance among the ancient Hebrews of Messianic hopes; in particular, of the belief in the day of Yahveh when the righteous who had descended to Sheol would arise and participate in the triumph of the nation. The faithful were to be resurrected, not in order to live a blessed, independent existence elsewhere than on this earth, but in order to be *reincorporated in the earthly life of the nation.* These were preliminary manifestations of needs which found their full expression in the modern conception of immortality.

The formation of that conception, as it took place among the Hebrews, is exceedingly interesting. Lack of space forbids anything more than a passing reference to some of the main facts. Job is an early shining instance among the Hebrews of a clear consciousness of the moral incompleteness involved in the limitation of human existence to earthly life. Yet he died without the hope of a blessed immortality. His nearest approach to it is a fleeting persuasion or hope that after death he would enjoy for a moment a vision of God, who would then vindicate his mysterious ways.

The transformation of Yahveh, the God of the nation, into a God maintaining individual converse with the members thereof, and holding each individual, and no longer the nation alone, as morally responsible to him, is intimately connected with the establishment among the Jews of the modern belief in immortality. The tragic inner life of Jeremiah shows us how circumstances forced him into individual relationship with Yahveh (chapters xv-xvii). Ezekiel continued the development of Jeremiah's thought. From the existence of an individual relationship with a just God, he drew the unavoidable conclusion that each individual is to be rewarded or punished according to his desert. This new doctrine permeates the Psalms and the book of Proverbs. But when limited to earthly existence, the doctrine is obviously false. Job and the author of Ecclesiastes are up in arms against this truncated truth: "All things come alike to all, there is one event to the righteous and to the wicked; to the good and to the clean, and to the unclean; to him that sacrificeth and to him that sacrificeth not; as is the good, so is the sinner; and he that sweareth, as he that feareth an oath." Ezekiel's doctrine could be made true only by positing another life after death in which the injustice of this life would be repaired. This has remained a chief argument of those believers in immortality who also believe in a benevolent and righteous Creator.

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[8] W. Ward Fowler, *loc. cit.*, pp. 386-388.

The conception of and the belief in a blessed future existence in which man's deepest and noblest yearnings are to be realized, followed upon the appearance of a deep sense of the worth of these cravings. Whenever, among peoples already familiar with the idea of soul or ghost, these cravings were sufficiently keenly felt, they seemed to have given rise to a belief similar to the Christian belief in immortality.

In Egypt in the religion of the sun-god, long before the book of Job was written, a glorious existence with the god had been conceived. In Greece, Plato taught a lofty doctrine of successive earthly incarnations for the gradual purification of souls from the pollution which comes to them from their association with matter. Ultimately souls entered the glorious world of pure spirits. But this doctrine did not originate with the Greek philosopher. He tells us himself that he got it from the Orphic priests. The Orphic cult was addressed to Dionysos by a sect that had evolved a definite system of religio-philosophic belief, the chief article of which was the double composition of man: one part mortal, coming from the Titans, the other divine. Man's task was to rid himself of the Titanic element, which corresponds to the body, in order to return pure to God. The deliverance of the soul could not be achieved suddenly not without the helping mediation of Orpheus, who, let it be noted, demanded a pure life as condition of salvation from rebirth.

The nature of the primary conception of continuation after death gives proof that, unlike the modern conception, it was not born of desires for the fulfilment in another existence of hopes frustrated on this earth. Had it had that origin, it would necessarily have been conceived of in a form designed to satisfy these desires. The nature of the belief and its universality among savages show it to have been imposed, regardless of man's feeling toward it, as irresistibly as the belief in the existence of any object present to the senses.

Differences in origin lead to differences in function. In the primary belief, the ghosts, even those of friends, are on the whole sources of anxiety and fear, and the relations maintained with them aim almost exclusively at warding off their interferences in human affairs. No one loves a ghost and, speaking generally, no one desires to become one. The modern belief is, on the contrary, a vivifying conviction or hope, calling forth the best that is in one's personality.

To consider these two conceptions as bearing to each other the relation of the seed to the fruit, is, therefore, to disregard their respective nature and function as well as their origin. In none of these respects have these conceptions anything essential in common. That is why the primary conception had to be discredited and discarded before the modern one of a glorious life, fulfilling the noblest human demands, could be formed and entertained.

STATISTICS OF CONTEMPORARY RELIGIOUS BELIEFS.

In Part II of my book, I attempted to discover what proportions of the members of a number of influential classes (physical scientists, biological scientists, historians, sociologists, psychologists, and college students of non-technical departments) believe in personal immortality and in the God whose existence is presupposed by all the organized religions, i. e., a God conceived of as acting upon the physical world or at least upon man, at man's request, desire, or desert. It appeared to me of great interest both practically and scientifically to find out definitely the percentages of believers, disbelievers, and doubters among these classes, and to correlate eminence in them and the special kinds of knowledge possessed by their members with these percentages.

I was aware that the statistics of belief so far gathered have little or no statistical value. When, as in the case of the extensive inquiry of the Society for Psychical Research, less than one-third of those who were solicited answered, no particular meaning attaches to the discovery that two-thirds of that one-third believe in immortality. In order to obtain statistics valid for the whole of a group, it is not necessary, it is true, to poll every member of the group. It is sufficient to consider a part of that group, provided that every member of that part or a very high percentage, answer the inquiry, and that the selection of the part investigated be made according to chance. The statistics of that part may then, according to the law of probability, be held valid for the whole group.

The statistical defect from which the inquiry of the Society for Psychical Research suffers, is often combined with an insufficient definition of the belief under investigation. Not long ago some rash person affirmed in the English press that "it is extremely doubtful whether any scientist or philosopher really holds the doctrine of a personal God." Thereupon a Mr. Tabrum collected from among English scientists 140 expressions of opinion on the question, "Is there any real conflict between the facts of science and the fundamentals of Christianity?" But the author did not define what he meant by "fundamentals," neither did he ask his correspondents to state the meaning they attached to that expression. Strange to say, very few thought it necessary to be explicit. Lord Rayleigh wrote, for instance, "I may say that in my opinion true science and true religion neither are nor could be opposed." This has the appearance of a misplaced pleasantry. Any one may make that statement; its significance depends altogether upon what is meant by "true religion." You may have in mind some conception of religion which would tolerate neither the Apostles' nor the Nicean creed, nor even a personal God!

In my own investigation I endeavored to avoid the two major defects illustrated above, and succeeded, I think, in establishing statistics of belief valid for the entire classes named above, so far as the United States is concerned.

The student of human development will be interested in the possibility now opened to ascertain the statistical history of religious beliefs. By instituting at some future time an investigation similar to mine, it would become possible to express with a high degree of exactness the changes that have taken place in the spread of the beliefs here considered.

If I cannot enter here into details as to the statistical method I have followed, the results secured, and their interpretation, I may at last add in conclusion the following figures and some brief information.[9]

BELIEVERS IN GOD	PHYSICAL SCIENTISTS	BIOLOGISTS	HISTORIANS	SOCIOLOGISTS	PSYCHOLOGISTS
Lesser Men	49.7	39.1	63.	29.2	32.1
Greater Men	34.8	16.9	32.9	19.4	13.2
BELIEVERS IN IMMORTALITY					
Lesser Men	57.1	45.1	67.6	52.2	26.9
Greater Men	40.	25.4	35.3	27.1	8.8

These figures show that in every class of persons investigated the number of believers in God is less, and in most classes very much less, than the number of non-believers, and that the number of believers in immortality is somewhat larger than in a personal God; that among the more distinguished, unbelief is very much more frequent than among the less distinguished; and finally that not only the degree of ability, but also the kind of knowledge possessed is significantly related to the rejection of these beliefs.

"The correlation shown, without exception in every one of our groups, between eminence and disbelief appears to me of momentous significance. In three of these groups (biologists, historians and psychologists) the number of believers among the men of greater distinction is only half, or less than half the number of believers among the less distinguished men. I do not see any way of avoiding the conclusion that disbelief in a personal God and in personal immortality is directly proportional to abilities making for success in the sciences in question.[10]

"With regard to the kinds of knowledge which favor disbelief, the figures show that the historians and the physical scientists provide the greater; and the psychologists, the sociologists and the biologists the smaller number of believers. The explanation is, I think, that psychologists, sociologists and biologists in very large numbers have come to recognize fixed orderliness in organic and psychic life, and not merely in inorganic existence; while frequently physical scientists have recognized the presence of invariable law in the inorganic world only. The belief in a personal God as defined for the purpose of our investigation is, therefore, less often possible to students of psychic and of organic life than to physical scientists.

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[9] These figures are percentages of the number of persons who answered the *questionnaire*.

[10] Concerning these abilities and their influence, see Chapter X.

"The place occupied by the historians next to the physical scientists would indicate that for the present the reign of law is not so clearly revealed in the events with which history deals as in biology, economics, and psychology. A large number of historians continue to see the hand of God in human affairs. The influence, destructive of Christian beliefs, attributed in this interpretation to more intimate knowledge of organic and psychic life, appears incontrovertibly, as far as psychic life is concerned, in the remarkable fact that whereas in every other group the number of believers in immortality is greater than that in God, among the psychologists the reverse is true; the number of believers in immortality among the greater psychologists sinks to 8.8 percent.

"One may affirm, it seems, that in general the greater the ability of the psychologist, the more difficult it becomes for him to believe in the continuation of individual life after bodily death.

"The students' statistics show that young people enter college possessed of the beliefs still accepted, more or less perfunctorily, in the average home of the land, and that as their mental powers mature and their horizon widens a large precentage of them abandon the cardinal Christian beliefs. It seems probable that on leaving college, from 40 to 45 percent of the students with whom we are concerned deny or doubt the fundamental dogmas of the Christian religion. The marked decrease in belief that takes place during the later adolescent years in those who spend those years in study under the influence of persons of high culture, is a portentous indication of the fate which, according to our statistics, increased knowledge and the possession of certain capacities leading to eminence reserve to the beliefs in a personal God and in personal immortality."[11]

To the statistical data are added a large number of letters from my correspondents and a somewhat full study of the religious ideas of students. These together with the statistics make a picture of the present religious situation both vivid and relatively exact.

J. H. Leuba

Bryn Mawr College.

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[11] *The Belief in God and Immortality*, pp. 277-281.

NOTES ON RECENT WORK IN THE PHILOSOPHY OF SCIENCE.

Federigo Enriques ("Sur quelques questions soulevées par l'infini mathématique," *Rev. de métaphysique et de morale,* March, 1917, Vol. XXIV, pp. 149-164) points out that experience, when it is idealized by reason, puts before us two kinds of infinity, the actually and the potentially infinite; suppose then "that we are potentially given by thought an infinity of objects, the question arises as to whether there is any reason to consider as logically defined a new object of thought which expresses the totality or the limit of these objects even when they are not constructed with respect to a concept of the kind which we suppose to be given *a priori.*" The answer to this question depends on a fundamental tendency of the mind; it will be negative or in some degree positive according as we feel ourselves borne toward nominalism or toward realism. Realist doctrine—at least in mathematics—in its first historical form rested on the assumption that a simple passage to the limit was always possible, and this was gradually destroyed by the progress of the infinitesimal calculus. The realist doctrine in its second historical form rests on the principle that (p. 159) "every infinity of virtually defined objects may be considered as a totality forming a class and constituting a new logical object." As distinguished from realism in its first form, in this new realism we conceive that the properties of the new object are absolutely new and that thus we cannot state them *a priori* by an induction extended from the finite to the infinite. This new form of realism is due, in its mathematical form, to Georg Cantor, but (p. 159) "the philosopher B. Russell has developed in the widest sense the philosophical consequences of the realism thus introduced into mathematics." The various paradoxes of mathematical logic have led to the conclusion that there are, in certain cases, no such things as classes of perfectly definite objects; and this realism, in its second form, is partially unsuccessful. On p. 163 we read that "the principle of an infinite number of choices is adopted by Russell and by Zermelo," and so we are apparently again forced to the conclusion that Enriques is quite unaware of the tendency shown by Russell's work published since 1905. Since the question is rather important, perhaps the present reviewer may be forgiven for dwelling on some work on the paradoxes in question since 1903.

In Russell's *Principles of Mathematics* (Cambridge, 1903) there was not any definite suggestion that the concept of class should be restricted, though there was certainly a more or less vague feeling that some classes should be excluded (see *Monist*, January, 1912, Vol. XXII, pp. 153, 157-158; and January, 1917, Vol. XXVII, p. 144). The merit of perceiving that a restriction was necessary and of attempting to give a criterion to decide which classes were legitimate seems due to Jourdain, in a paper published in 1904 (see *Monist*, January, 1917, Vol. XXVII, pp. 148-150). The question as to the *being* or *not-being* of a class is totally different from the question of the possibility of an infinite series of acts of selection,—which, by the way, neither was nor is assumed by Russell, though it is believed in by Zermelo and many others. The merit of being the first to publish an explicit recognition of the postulate involved in this assumption is due to Zermelo in 1904, and the questions relating to Zermelo's axiom have been frequently confused with, for example, Jourdain's "proof" by even eminent people; though both Russell and Jourdain pointed out repeatedly that the questions involved are quite different. In 1905 and later Russell published papers gradually showing how it was possible to work through a great deal of Cantor's theory without assuming that there are such things as classes at all, and a thorough exposition of this theory is one of the most important parts of Whitehead and Russell's *Principia Mathematica* (Vol. I, Cambridge, 1910). Thus it is obvious that this theory of Russell's not only makes the considerations of Jourdain and some others quite superfluous, but also makes such criticisms as that of Enriques entirely off the point (cf. *Monist*, January, 1917, Vol. XXVII, pp. 145-148).

This historical sketch is also relevant to our consideration of a recent paper by Dmitry Mirimanoff ("Les antimonies de Russell et de Burali-Forti et le problème fondamental de la théorie des ensembles," in *L'enseignement mathématique*, 1917, Vol. XIX, pp. 37-52). The author remarks (p. 48) that we can find in the works of Bertrand Russell, Henri Poincaré, and Julius König (*Neue Grundlagen der Logik, Arithmetik und Mengenlehre*, Leipsic, 1914) "a profound logical and psychological analysis of the Cantorian antinomies and of the notion of class," but that he "will not have any need of this analysis for the end which" he has in view. His article is characterized by the following quotation from p. 38: "People believed, and it seemed quite evident, that the existence of individuals necessarily implies that of the class of them, but Burali-Forti and Russell showed by different examples that a class of individuals may be non-existent, although the individuals exist. As we cannot refuse to accept this new fact, we are obliged to conclude from it that the proposition which seemed evident and which was believed always to be true is only true under certain conditions. And then arises the problem which we may regard as the fundamental problem of the theory of aggregates: What are the necessary and sufficient conditions for the existence of a class of individuals?" On this confusion between the ideas of existence and entity, see the article quoted above in *The Monist* for January, 1917. The author gives a solution of this problem for the particular case of classes that he calls "ordinary" classes, and his deductions rest on three postulates (p. 49) which are applied by some in the study of problems of the theory of aggregates. Further, the examples of Russell and Burali-Forti are modified (pp. 39-48) in a way that seems advantageous to the author, and the author announces (pp. 39, 52) his intention to give in a future article the reasons which determined him not to adopt in this paper the theory of König. The criterion which the author arrives at (pp. 48-52) for deciding whether individuals have a class or not is practically that suggested by Jourdain in 1904: individuals have a class if they can be arranged in a segment of the series W of all ordinal numbers and not if they cannot be so arranged. It is not worth while to enter into a criticism of this suggested criterion, which in any case has become quite superfluous through the work of Russell referred to above. Mirimanoff (p. 52) regrets that it has been impossible for him to become acquainted with work that has appeared since the beginning of the war. If he had read—which he nowhere gives any sign of having done—the works referred to above of 1904 to 1910, we do not think that it would have been necessary to write this paper.

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In the *Revue de métaphysique et de morale* for January, 1917, there is an address given by the late Victor Delbos on the general characteristics of French philosophy. A part of the late Louis Couturat's *Manuel de logistique,* which he wrote about 1906 but which is not yet published, is printed. These extracts form the greater part of the second chapter of this book, and are on the logical relations of concepts and propositions. An interesting fact about them is that the work of Frege seems to have influenced Couturat to a greater extent than was the case with Couturat's *Principes* of 1905. This contribution is fairly elementary, and does not deal with those paradoxes which are, perhaps, of the greatest interest to logicians, although it just mentions them. F. Colonna D'Istria writes on the logic of medicine according to Cabanis's *Rapports du physique et du moral de l'homme*. Arnold Reymond makes a critical study of the new and recast edition of Edouard Claparède's *Psychologie de l'enfant et pédagogie expérimentale* (Geneva, 1916). Thomas Ruyssen writes on "an idea in peril: humanity, humanitarianism, humanism." Finally there are obituary notices of Théodule Ribot (1839-1916), the eminent psychologist, and Henri Dufumier.

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In the number of the *Revue de métaphysique et de morale* for May, 1917, A. Espinas deals with the initial idea of the philosophy of Descartes, and the late Victor Delbos's lecture on method in the history of philosophy forms the second of his three lectures on the history of philosophy. A manuscript by the late Louis Couturat, which was certainly written before 1902 and which will not form part of the projected *Manuel de logistique*, is printed here and is on the algebra of logic and the calculus of probabilities. Finally Alessandro Padoa has a paper on the consequences of a change of primitive ideas in any deductive theory whatever.

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In a paper on the "infinite numbers" which Bernard le Bovier Fontenelle tried to introduce in his *Eléments de la géométrie de l'infini* (Paris, 1727), Branislav Petronievics ("Sur les nombres infinis de Fontenelle," *Rendiconti della R. Accademia dei Lincei* [*Classe di scienze fisiche, matematiche e naturali*], Vol. XXVI, 1917, pp. 309-316) tries to show that this "first attempt at a rational theory of infinite numbers," although it is obviously full of contradictions which were at once pointed out by MacLaurin and others, possesses a historical value when compared with the theories of Cantor and Veronese. Cantor's theory has, says Petronievics, an arithmetical starting-point, while that of Veronese has a geometrical one; and Veronese establishes that there is no point on an infinite straight line which corresponds to the first transfinite ordinal number of Cantor, so that "geometrical application of the transfinite numbers of Cantor is not possible." In spite of the fact that Fontenelle introduced his "infinite number of all finite numbers" as "the last one" of this series itself, Petronievics maintains that the theory of Fontenelle has a historical value in that it "may be regarded as the common source of the theories of Cantor and Veronese," and that "it is not impossible to suppose, in view of the likenesses between the theories, that Cantor and Veronese both arrived at establishing the principles of their theories when trying to avoid the flagrant contradictions into which Fontenelle fell."

There does not seem to the reviewer to be the smallest ground for supposing that Cantor was led to his theory either by reading Fontenelle or by setting out deliberately to generalize arithmetic. Indeed, one of the points of the long introduction to the translation of Cantor's later papers published under the title of *Contributions to the Founding of the Theory of Transfinite Numbers* (Chicago and London, 1915) is to show that Cantor was compelled to generalize the idea of number as a consequence of the natural development of his process of "derivation" of geometrical point-sets. In this extension it appeared clearly that the transfinite numbers began *beyond* the whole series of finite numbers in opposition to Fontenelle's notion mentioned above. Fontenelle says on page 30 of his book: "We must not be frightened at the words 'last term' in this connection. It is a last *finite* term that the natural series of numbers has not, but not to have a last finite term is the same thing as to have a last infinite term." This is a charming way of turning a universal negative proposition into a particular affirmative one. It seems that the first time that Cantor spoke more or less publicly of Fontenelle's theory was in a letter of 1886 (cf. *Zur Lehre vom Transfiniten,* Halle, 1890, p. 50), and therefore long after he had founded his absolutely different theory. That hearing of one theory may have been the psychological cause of Cantor's thinking about a fundamentally different theory is of course both possibly and probably irrelevant, but there is no ground for supposing that even this happened. It is a mistake to say that ordinal numbers cannot have geometrical applications: an illustration of the way such numbers can appear is given by this: To the series on the x-axis formed by the points 1, 1/2, 1/3, ..., $1/n$,...., the point 0 bears exactly the same relation as the first transfinite ordinal does to the finite ordinals in order of magnitude. For Cantor's remarks on Veronese and Veronese's reply, we may quote the above

Contributions (pp. 117-118). A purely analytical exposition of the infinite and infinitesimal numbers of Veronese was given by T. Levi-Civita ("Infiniti e Infinitesimi attuali," *Atti R. Istituto Veneto*, 1892).

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Florian Cajori ("The Zero and Principle of Local Value used by the Maya of Central America," *Science*, Vol. XLIV, 1916, pp. 714-717) draws attention to the fact, hitherto apparently unnoticed by mathematicians, that the Maya of Central America and southern Mexico seem to have used a symbol for zero and the principle of local value much earlier than any one else. The material for Cajori's remarks is furnished by *An Introduction to the Study of the Maya Hieroglyphs*, by Sylvanus Griswold Morley (Bulletin 57 of the Bureau of American Ethnology, Washington, 1915). The early Babylonians possessed the principle of local value, but so far as we know did not possess a zero. About 200 B. C. they did have a symbol for zero, which, as Smith and Karpinski (*Hindu-Arabic Numerals*, Boston, 1911, p. 51) say, was "not used in calculation, nor does it always occur when units of any order are lacking." They did not employ it systematically in writing numbers and not at all in performing computations. The Hindus certainly did not use their symbol for zero systematically before probably the sixth century A. D., and the earliest undoubted occurrence of zero in Indian numerals is A. D. 876 (cf. also G. R. Kaye, *Indian Mathematics*, Calcutta, and Simla, 1915, p. 31, for the date of the appearance of the principle of local value in India). Now, it seems that the Maya used the zero and the principle of local value at the beginning of the Christian era if not much earlier. "As far as is known, the Maya used their numeral systems only in the counting of time as it arose in their calendar, ritual, and astronomy." Of the several Maya numeral notations the one which is of greatest interest as embodying the principle of local value and the symbol for zero is found in Maya codices but not in their inscriptions. The number system was vigesimal, with the solitary break that 18 (and not 20) *uinals* make 1 *tun*, and the symbols 1 to 19, both inclusive, are expressed by bars and dots. Each bar stands for five units and each dot for one unit, and the dots are written above the bars. Thus 19 is written as three bars above one another and four dots on the top. "The values of the bars and dots are *added* in each case. The zero, which plays a leading part in the notations found on inscriptions as well as those on codices, is represented in the codices by a symbol that looks roughly like a half-closed eye.... In writing 20... the principle of local value enters for the first time. It is expressed by a dot placed over the symbol for zero. The numerals are written, not horizontally, but vertically, the unit of the lowest order or value being assigned the lowest position. Accordingly, 37 was expressed by the symbols for 17 (three bars and two dots) in the *kin* [units] place and one dot, representing 20, placed above the 17, in the *uinal* place. The number 300 is expressed by three bars drawn above the symbol for zero $(3 \times 5 \times 20 = 300)$. The largest number which can be written by the use of only two places or positions is $17 \times 20 + 19 = 359$. To write 360, the Maya drew two zeros, one above the other, with one dot higher up, in third place. Using three places to represent *kins, uinals,* and *tuns,* they could write any number not larger than 7199. Proceeding in this way the Maya wrote numbers in very compact form. The highest number found in codices is 12,489,781.... The symbols representing this number occupy six different places, one above the other.... The second numeral notation that was fully developed and employed by the Maya is found in their inscriptions. It employs the zero, but not the principle of local value. Special glyphs are employed to designate the different units. It is as if we were to write 1203 as: '1 thousand, 2 hundreds, 0 tens, 3 ones.'"

The question as to the origin of the arithmetical notation that we call "Hindu-Arabic" has received a new and unexpected contribution from Carra de Vaux ("Sur l'origine des chiffres," *Scientia,* Vol. XXI, 1917, pp. 273-282.) With the Arabs these figures are called *hindi* and the usual meaning of this word is "Indian." Now, the Arabian historian Masoudi, writing in 943 A. D., said that the Hindu numerals were discovered by a congress of wise men gathered together by the powerful and wise king Brahman under whom arts and sciences flourished. "People who are even slightly familiar with the history of philosophy will recognize this at once as a neo-Platonic legend." and a mention of the "Era of the Creation" allowed de Vaux to conclude that this legend is Persian, for that is a Persian era. Also in the work of the other Arabic historian, Albîrouni, we have a remark that the numerals came from India, that is vague and contrasts strongly with his usual exactness. This seems to show a lack of definite knowledge on Albirouni's part.

The author then examines the word *hindi,* and comes to the conclusion that it is a form of *hindasi* whose root is the Persian *ènd* and which means metrical or arithmetical. Thus "signs of *hind*" means "arithmetical signs" and not "signs of India." Consider this example: Apollonius of Perga, who was not an Indian, was said to be *el-hindi* in some Arabic manuscripts, thus this word must evidently be translated as if it were *el-hindasi,* the geometer or engineer. It is to be noticed that in Arabian treatises the abacus is called *takht* which is a Persian name. Thus de Vaux concluded that the numerals originated in the Greek world, and the history of their slow diffusion is easier to explain if we admit that they are a neo-Platonic "or (*soit*) neo-Pythagorean" invention, for the Pythagoreans are well known to have had a taste for secrecy. From Greece the numerals passed to Persia and the Latin world, and from Persia to India and afterward to Arabia.

The figures themselves were not formed from letters of the alphabet, but directly by means of very simple conventions. These characters are due to the neo-Platonists and were known in the schools of Persia before they were in Islam, and it is there that the Arabs found them. From Persia also they passed into India.

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In the first number (January, 1917) of Vol. XXIV of the *American Mathematical Monthly,* the official journal of the Mathematical Association of America, there is an important paper by Edward V. Huntington on "The Logical Skeleton of Elementary Dynamics" (pp. 1-16). The object of the article is to outline the logical structure of elementary dynamics. Any logically developed science must begin with undefined concepts, in terms of which all the other concepts of the science are expressed; and in this case Huntington takes them to be: (1) Space and time, with the derived concepts of velocity and acceleration; (2) Forces, "as suggested by the tension and compression in our own muscles" (p. 1) and "as measured by a spring balance" (p. 3); and (3) Inert material bodies, on which our forces act. The unproved propositions, from which all the other propositions of the science are derived, are only four in number. The first is (p. 4) that "a free particle, when acted on by a force, acquires an acceleration in the direction of the force; furthermore, if a given particle is acted on at different times by two forces F and F′, and if a and a' are the corresponding accelerations, then $F/F' = a/a'$; that is, the accelerations are proportional to the forces." This principle "is best regarded as a scientific hypothesis, the truth of which has been abundantly verified by experiment." It contains the answer to the fundamental question of dynamics: "If a force gets hold of a free particle, and proceeds to act on it, what happens to the particle?" The second and third principles, which cover the case of the particle being acted on by several forces simultaneously, are the principle of the vector addition of forces and the principle of the independence of two perpendicular forces (p. 5). For any given body the ratio of the force to the acceleration produced is constant, and the value of this ratio "is a characteristic of the body, which may be called its inertia" (p. 4). "The *weight* of a body in a given locality with respect to a given frame of reference is best defined as the force required to support the body at rest with respect to that frame in the given locality" (p. 5). We then have the theorem that "if W is the weight of a body in a given locality and g is the falling acceleration of that body in the same locality, then the ratio W/g is independent of the locality, and is a correct expression for the inertia of the body" (p. 6). The proof for the case of fixed axes follows immediately from the first two principles, and a proof for the case of moving axes "belongs later in the course." The theorem (p. 6) that, in any given locality, the falling accelerations of all bodies are equal, "can be proved from general considerations; or, if preferred, it may be accepted as an empirical fact." The words "mass of three pounds" are taken (p. 7) as meaning the same thing as that the body in question "has a weight of 3 lbs." in the standard locality; the weight being a multiple of the unit of force (lb. in the British system). The fourth and last fundamental principle is the principle of action and reaction (p. 8): "When two particles are in contact with each other, or attract or repel each other according to any law like that of gravitation or magnetism, the interaction between them may be represented by a pair of twin forces, equal in magnitude and opposite in direction—one of the twins acting on one particle and one on the other, along their joining line." The definition of the "centroid or center of mass" is as a "weighted average" (p. 8), and the theorem on the motion of the center of mass is then proved. It is emphasized (p. 11) that the difficulties outside the four fundamental principles are of a mathematical sort. It will be seen that the system is based on fundamental units of force, length, and time instead of on units of mass, length, and time, and the author shows by tables (pp. 15, 16) the higher practical value of the system of derived units advocated by him. This is "one of the best arguments in favor of the use of force rather than mass as the principal undefined concept of dynamics. The only reason why the text-books so insistently base their derived units on mass instead of on force is apparently that a standard lump of metal is easier to preserve in a museum than a standard spring balance. But this is no argument for the logical priority of mass over force. As a matter of fact, the fundamental unit of force is as easy to preserve as the fundamental unit of mass, though the method of doing so does not consist in simply storing away a spring balance" (p. 16). The name of the unit of force, in the British system, is "pound" (lb.) and is defined as "the force required to support a carefully preserved lump of metal, called the 'standard pound avoirdupois,' in vacuo, in the standard locality" (p. 14).

The reviewer would remark that, though mass under the name "inertia" is a derived unit in the system advocated by Huntington, we have to use the unit of mass as a practical means of preserving the unit of force. It is quite true that this fact is no argument for the logical priority of mass: it is merely a question of practical convenience. But in either of the two systems there seem to be, at first sight, three fundamental undefined units, and so, from a logical point of view, nothing is gained by replacing mass by force as a fundamental unit. But let us look at the matter more closely. As we have learned from the work of Mach (see, e. g., his *Mechanics,* 3d edition, Chicago and London, 1907, p. 243), "mass-ratio" can be defined in terms of the mutual accelerations of bodies, and so there seems to be a logical advantage in the system in which force is not regarded as fundamental, but is defined. Further, even in Huntington's system, "force" can be defined by the property that F/a is constant, and then his system and Mach's seem to be identical. By the way, the forces we use in dynamics are not all "suggested by the tension and compression in our own muscles": the attraction of the sun is not; and it is both logically objectionable and rather confusing to a student to have various concepts with a single name.

It must be added that stress is (pp. 7-8) rightly laid on the difficulty which beginners have in realizing that, when a particle describes a curve, there is actually an acceleration along the normal.

There is an interesting review of Florian Cajori's *William Oughtred* on pp. 29-30 written by Louis C. Karpinski, where it is stated that Cajori's remark that Napier was the first to use a decimal point (1616 and 1617) is an error: it was first used by Pitiscus in 1612.

In the February number is a short account (pp. 54-55) of Cajori's presidential address to the annual meeting of the Mathematical Association of America in New York City at the end of 1916 entitled "Discussions of Fluxions from Berkeley to Woodhouse." This address was a shortened account of a book by Cajori which will appear before very long in the "Open Court Classics of Science and Philosophy." At a meeting of the Council it was decided, among other things, to consider the question of possible assistance for the *Revue semestrielle* and the *Jahrbuch über die Fortschritte der Mathematik* in view of the difficulties that must attend publication owing to the war (p. 64). Very much the same discussion was held by the Chicago Section of the American Mathematical Society (p. 97). David Eugene Smith, in "On the Origin of Certain Typical Problems" (pp. 65-71), has a very learned article on the history of the problems of (1) filling a cistern of water, (2) the *Josephspiel*, or the problem of the Turks and Christians, (3) the testament of a man about to die, dividing his estate, (4) the problem of pursuit.

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Frank Egleston Robbins (*Amer. Math. Monthly*, March, 1917, Vol. XXIV, pp. 121-123) gives an interesting and critical review of George Johnson's partial translation of and commentary on the *Introduction to Arithmetic* of Nicomachus, in his dissertation on *The Arithmetical Philosophy of Nicomachus of Gerasa* (Lancaster, Pa., 1916). The essay of Nicomachus is of course the earliest extant attempt at a systematization of the Greek science of theoretical, as distinguished from practical, arithmetic.

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In the *American Mathematical Monthly* for May, 1917, W. H. Bussey ("The Origin of Mathematical Induction," Vol. XXIV, pp. 199-207) points out that Moritz Cantor is mistaken about the use of complete induction both in his *Geschichte der Mathematik* (Vol. II, 2d ed., 1900, p. 749) and his note, correcting this mistake, on Maurolycus in the *Zeitschrift für mathematischen und naturwissenschaftlichen Unterricht* (Vol. XXXIII, 1902, p. 536). In the note Cantor said that he had found that Maurolycus described and used the method in his *Arithmeticorum libri duo* (Venice, 1575), and that Pascal had expressly borrowed the method from Maurolycus; Bussey shows that there is an error, in a minor respect, in Cantor's references. Bussey quotes a number of Maurolycus's theorems. In his sixth proposition, that any integer (n) added to the preceding one is equal to the "collateral odd number" (the nth odd number, $2n-1$), Maurolycus's proof, freely translated is: "The integer 2 added to unity makes the integer 3, but when added to 3 it makes an amount greater by 2 and this.... is the next odd integer, namely 5. Again, since the integer 3 added to 2 makes 5, which is the collateral [third] odd integer, when it is added to 4 the result will be greater by 2, that is....it will be the next odd integer, which is 7. And in like manner to infinity as the proposition states." On this proof Bussey remarks (p. 201): "This is not a very clear statement of a proof by mathematical induction but the idea is there." The eleventh proposition, that every triangular number added to the preceding triangular number is equal to the collateral square number, or, in modern notation, $n(n+1)/2+(n-1)n/2=n^2$, is the one which Cantor said, in the above note, Pascal got from Maurolycus and which Maurolycus proved by complete induction. But Cantor is mistaken in saying that this theorem is proved by complete induction: the first undoubted case of a proof by complete induction is the fifteen proposition, that the sum of the first n odd integers is equal to the nth square number. Maurolycus's proof is (p. 203): "By a previous proposition the first square number (unity) added to the following odd number (3) makes the following square number (4); and this second square number (4) added to the third odd number (5) makes the third square number (9); and likewise the third square number (9) added to the fourth odd number (7) makes the fourth square number (16); and so successively to infinity...." Pascal mentioned in a letter to Carcavi the fact that he borrowed from Maurolycus, and he repeatedly used the method of complete induction in connection with his arithmetical triangle and its applications. Bussey then gives two interesting examples of Pascal's use of the method of complete induction, and finally gives some other and more recent uses of it.

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In the same number of the *Monthly*, David Eugene Smith ("Mathematical Problems in Relation to the History of Economics and Commerce," pp. 221-223) maintains that "a very good history of civilization could be written from the wide range of problems of mathematics." In the subject of commercial and economic history, for example, he mentions that the problems in the manuscripts and early printed books on arithmetic in the fifteenth century tell us that Venice was then the center of the silk trade, although Bologna, Genoa, and Florence were then prominent; the problems also tell us the cost of the luxuries and necessities of life; the rent of houses; the changes in commercial customs and the rise in standards of business integrity. "Not only to the economist and the student of commerce is the field a rich one, but it is well worth the study of any one who may be possessed of doubt as to the relation of mathematics to the daily life of the race. Not only can the history of the problem easily be made the history of commerce and economics, but the history of mathematics can easily be made the history of civilization."

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In the number of the *Bulletin of the American Mathematical Society* for March, 1917 (Vol. XXIII), there are two interesting papers by Edward V. Huntington on the logical postulates for order. In "Complete Existential Theory of the Postulates for Serial Order" (pp. 276-280) Huntington establishes the "complete independence"—in the sense defined by E. H. Moore of Chicago in his *Introduction to a Form of General Analysis* of 1910—of each of three different sets of postulates for serial order. The first set is new and very convenient for many purposes; the second set dates back to Vailati (1892); the third set is a modification of the second set and was introduced in Huntington's well-known paper on "The Continuum as a Type of Order" in the *Annals of Mathematics* for 1905. In "Complete Existential Theory of the Postulates for Well Ordered Sets" (pp. 280-282) Huntington gives three sets of independent postulates for well-ordered systems, each of these three sets being "completely independent" in the above sense. R. L. Borger ("A Theorem in the Analysis of Real Variables," pp. 287-290) gives a theorem on two real functions of two real variables which is derived from a theorem in Kowalewski's *Die komplexen Veränderlichen und ihre Funktionen,* and deduces from it the exceedingly fundamental and important theorem that if any function of a complex variable possesses a finite derivative at each point of a simply connected closed region, then this derivative is continuous, all the derivatives of the function exist, and the function may be represented by a power-series. Mathematicians who are acquainted with the nature of the progress brought about by Goursat's proof of Cauchy's theorem will at once see how important this note is. J. R. Kline ("Concerning the Complement of a Countable Infinity of Point Sets of a Certain Type," pp. 290-292) proves a theorem which is a general case of the theorem proved by Hausdorff in his *Grundzüge der Mengenlehre* of 1914 that, if E denotes a Euclidean space of two or more dimensions while R is an enumerable set of points belonging to E, then E – R is a connected set. Kline's theorem was proved by Robert L. Moore (*Trans. Amer. Math. Soc.* for 1916) on the basis of a system of axioms proposed by him.

* * *

The number of the *Bulletin of the American Mathematical Society* for May, 1917 (Vol. XXIII, No. 8), contains several articles of interest to those who cultivate the philosophical and historical aspects of mathematics. Samuel Beatty ("The Inversion of an Analytic Function," pp. 347-353) proves the existence of the inverse of an analytic function when the conception of an analytic function which is due to Goursat is the starting-point. In the theory of Weierstrass this proof is made to depend on the representation by a series of powers and in Cauchy's theory on the Jacobian of the real and imaginary parts of the function with reference to the real and imaginary parts of the variable. It is well known that Goursat showed in 1900 how the fundamental proposition on complex integration in Cauchy's theory could be proved merely from the assumption that the function in question has a finite derivative at each point of a simply connected domain, without any assumption of the continuity of this derivative. This continuity was then proved as a consequence of the Cauchy-Goursat theorem. The method of Beatty's proof makes use of the theory of sets of points. Thomas S. Fiske ("Emory McClintock," pp. 353-357) gives a biography of Emory McClintock (1840-1916). McClintock's first paper on pure mathematics entitled "An Essay on the Calculus of Enlargement," in the *American Journal of Mathematics* for 1879 "was an effort to present the theory of finite differences and the differential calculus from a unified point of view. The paper may be regarded as a precursor of recent attempts to consider difference equations as differential equations of infinite order. His other more important papers were a series of researches on solvable quintic equations published in the *American Journal of Mathematics* [for 1884, 1885 and 1898] and a paper on the theory of numbers ['On the Nature and Use of the Functions Employed in the Recognition of Quadratic Residues'] published in the third volume [1902] of" the *Transactions of the American Mathematical Society* (p. 355). "When one considers that McClintock made no use of the powerful labor-saving machinery which has revolutionized modern analysis, the results obtained by him in his researches on quintic equations, as well as some of his other achievements, appear to indicate a truly wonderful power of manipulation and clearness of vision" (p. 356). A list of McClintock's mathematical publications is given.

J. H. Weaver ("On Foci of Conics," pp. 357-365) gives (1) a short historical sketch of the development of the properties of conics connected with the foci, and (2) some of the theorems from Pappus which have a bearing on foci and tangents. According to Zeuthen (*Geschichte der Mathematik im Alterthum und Mittelalter*, Copenhagen, 1896, p. 211) it seems that the focus for the parabola may have been known to Euclid. However, we have no mention of such points or of any of their properties until the work of Apollonius on conics (Book III, Probs. 45-52), but Apollonius did not use or mention in any way a focus for the parabola. Pappus gave the first recorded use and proofs of the focus-directrix definition of conics. Johann Kepler named the points in question in a work of 1604, and part of the short account of the conic sections that he gave (*Opera Omnia*, ed. Frisch, Frankfort, 1859, Vol. II, p. 185) is freely translated by Weaver (p. 359) as follows: "There are among these curves certain points of especial consideration, which have a certain definition but no name, unless they usurp for name the definition of some property. For if from these points lines are drawn to the points of contact of tangents to the section, these lines make equal angles with the tangents....We, because of the properties of light and the eye, from the viewpoint of mechanics shall call these points *foci*. We might have called them *centers*, because they are on the axis of the section, if authors, in the hyperbola and ellipse, were not accustomed to calling another point the center. In the circle there is one focus, the center. In the ellipse there are two foci equally distant from the center, and more removed in the more acute. In the parabola, one focus is within the section and the other may be considered either within or without the section and removed to an infinite distance from the first focus, so that if a line drawn from this *caecus* [blind] focus to a point of the section will be parallel to the axis. In the hyperbola, the external focus becomes nearer the internal focus as the hyperbola becomes more obtuse." The method of the work of Kepler was developed and added to by Desargues, and important work on foci was done by Maclaurin, Poncelet, Plücker, and many others.

Finally, there is a short and extremely interesting paper by Jekuthial Ginsburg ("New Light on Our Numerals," with an introductory note by David Eugene Smith, pp. 366-369). That our common numerals are of Hindu origin seems to the author to be a well-established fact, and that Europe received them from the Arabs seems equally certain, but how and when these numerals reached the Arabs is a question that has never been satisfactorily answered. The article calls attention to a paper by the French orientalist F. Nau in the *Journal asiatique* for 1910 (Series X, Vol. XVI) showing that the Hindu numerals were known to and appreciated by the Syrian writer Severus Sebokht who lived in the second half of the seventh century; that is, about one hundred years before the date of the first definite trace that we have hitherto had of the introduction of the system into Bagdad. Sebokht says, after asserting that the Greeks, in astronomy, were merely the pupils of the Babylonians: "I will omit all discussion of the science of the Hindus, a people not the same as the Syrians; their subtle discoveries in this science of astronomy, discoveries that are more ingenious than those of the Greeks and the Babylonians; their valuable methods of calculation; and their computing that surpasses description. I wish only to say that this computation is done by means of nine signs. If those who believe, because they speak Greek, that they have reached the limits of science should know these things they would be convinced that there are also others who know something" (p. 368). On this fragment Ginsburg remarks (pp. 368-369) that it "clearly shows that not only did Sebokht know something of the numerals, but that he understood their full significance, and may even have known the zero as Rabbi ben Esra did, in spite of the fact that he, too, speaks of nine numerals." However, Smith (pp. 366-367) remarks that the article "shows that the zero was probably not in the system as then mentioned, showing at least that its value was not generally comprehended in the seventh century and possibly confirming the impression that the symbol had not yet been invented." With regard to the question as to how Sebokht could have obtained information about the Hindu numerals, Ginsburg remarks (p. 369) that the city where Severus lived, in the northeast part of Mesopotamia, "was situated in a rich and fruitful country, was long the center of a very extensive trade, and was the great northern emporium for the merchandise of the east and west;" and "the exchange of goods is always accompanied by the exchange of ideas." Further, the weight of the evidence is (p. 369) in favor of Sebokht's work being at least one of the agencies by means of which the knowledge of the numerals was transmitted to the Arabs.

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Raphael Demos ("A Discussion of a Certain Type of Negative Proposition," *Mind*, Vol. XXVI, 1917, pp. 188-196) applies to particular negative propositions the treatment which Bertrand Russell (cf. Russell and Whitehead's *Principia Mathematica*, Vol. I, Cambridge, 1910) has applied to "descriptive phrases" or "incomplete symbols." Russell "found himself confronted with the fact that to accept descriptive phrases as significant in their given form would be to people the world of things with the apparent objects of such self-contradictory and fantastic descriptions as 'round-square,' 'centaur,' etc."; and Demos "was faced with the fact that to accept negative propositions at their face value would be to people the world of objects with negative facts, a type of objects which experience fails to disclose." Demos, somewhat like Russell, by viewing the negative proposition as an incomplete symbol, was led to declare it meaningless in its apparent form, and its apparent object —the negative fact—to be nothing. In this article he stated, first, that a particular simple negative proposition is an objective entity whose peculiarity as negative is not dependent upon the mind's attitude toward it. He then argued that the negative proposition cannot be construed in the form which it apparently possesses, inasmuch as such construction would make it formally different from positive propositions and would endow it with purely negative objects, which are, it seems, nowhere to be found in experience. He concluded that some special interpretation must be given to the negative proposition, and showed that its negative element is a modification, not of any distinct constituent (such as the predicate) in the proposition, but of the whole content of it. Thus any negative proposition is a modification, in terms of "not," of the rest of its content, and—since the latter is positive—a modification of some particular positive proposition. He stated the meaning of "not" to be "opposite"—a relational qualification in terms of the familiar relation of opposition or contrariety among positive propositions—and hence the meaning of the whole proposition "not-p" to be "opposite of p." He argued that, so stated, a negative proposition is an ambiguous description of some positive proposition, and that, completely stated, it is of the form "an opposite of p is true," or "some q is true which is an opposite of p." Thus he defined a particular simple negative proposition as an ambiguous description of some true positive proposition in terms of the latter's relation of opposition to a certain other positive proposition, such that, in terms of the former, reference is achieved to the latter. Lastly, he explained that negative knowledge is knowledge of a true positive proposition by description in terms of its opposition to some other proposition, and hence must be characterized as positive in reference but not in content, inasmuch as the proposition referred to is not a constituent of the complex of assertion or knowledge. "Substantially the above definition of simple negative propositions applies to double and 'n-ple' negatives as well; the latter, too, are descriptions of positive propositions which are true in terms of what they oppose. There is this difference, however, that whereas simple negatives are functions of a positive content, double and other negatives are functions of a negative content, such that any negative proposition in the nth power is a function of a content which is negative in the $(n-1)$th power." Φ

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C. E. Hooper publishes "The Meaning of the Universe" (*Mind*, April, 1917), the first instalment of an article of massive appearance. The definition is as follows: the *Universe* means the totality of real thought-objects (or object-matters) considered under four related aspects: (1) space, (2) time, (3) the variety in unity of natural characters, i. e., real thought-objects as particulars having natures of their own, but natures agreeing in various specific and generic respects with the natures of other particulars, (4) unity in variety of natural causation. Time and space are both objective. Mr. Hooper goes on to define thought-object, reality and aspect. A thought-object is apparently an *intended* object, whether or not a reality corresponds to the intention (e. g., Kant's *noumenon*). Reality is contrasted not with appearance but with "mental figment," and includes subsistent as well as existent objects. It is difficult to tell how far the term "thought-object" has an idealistic bias in Mr. Hooper's mind, but reality, at all events, seems to be merely a sum or system of objects which are severally real. The universe is thus a real thought which contains all other real thought-objects in their manifold relations. *Symbolic* entities (ideas, signs) are comprehended, but whether *per se* or only as reflected upon (made objects of thought) is not stated. It would seem that imaginary or inconceivable thought-objects, such as Meinong's pets, the golden mountain and the round square, are to have no place in the universe, but are discarded as "figments." While the universe contains finite thought-objects and symbols, it does so only in fact, not in nature. Of the four modes or aspects, space and time may be classed together as *coincidentals,* while the systems of natural characters and natural causation may be termed *co-essentials.* On the other hand space and nature may be classed together as static, time and causation as dynamic. We find some difficulty in understanding how Mr. Hooper accounts for the universe's being known at all. "It cannot, like a finite object, be actually related to some fellow object. It is as related to the mind or system of subjective ideas that we know all that is possible to know about it." But the "mind" if genuinely symbolic is a thought-object; and if the universe cannot be related to a finite object which is part of itself we do not know how it can be related to the mind. But criticism of so substantial an article should be deferred until its completion in succeeding issues. *η*

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Agnes Cuming ("Lotze, Bradley, and Bosanquet," *Mind*, April, 1917) declares Lotze's logic to be a partial revolt against the intellectualism of Hegel. Our intelligent experience, according to Lotze, is only a small part of the real world and thought is only a small part of our intelligent experience. Thought is a tool, a substitute for adequate perceptive intuition. Bradley's and Bosanquet's logic are similar in so far as each is influenced by Lotze. They hold an almost identical definition of "idea," and agree in their theories of judgment. Bradley however arrives at reality ontologically and Bosanquet epistemologically. "Knowledge for Bosanquet is the system of reality progressively demonstrated before our eyes.... In this emphasis on system as the postulate of knowledge....Bosanquet is in advance of Bradley." Lotze insists on feeling as a criterion, and is thus very far from Bosanquet with his conception of system, but he admits the essential of Bosanquet's position, which is the inadequacy of feeling. Lotze is a dualist: he divides sharply the feeling which supplies the material from thought, exercising a formal activity upon it. In Bradley the dualism becomes a gloomy scepticism; thought and its object are forever sundered. But Bosanquet bridges the gulf. In both Bosanquet and Bradley the separation of thought and reality is inherited from Lotze with his idea of the "scaffold" of thought. The only possible criterion of knowledge is immanent—a criticism of a lower from a higher point of view. Miss Cuming considers that Bosanquet has improved upon both Lotze and Bradley; the direction which she believes to be progress seems to be almost a return to more orthodox Hegelianism.

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In the same number B. M. Laing ("Schopenhauer and Individuality") considers that Schopenhauer fails to appreciate the metaphysical claims of individuality. He dethrones reason, making it a mere temporary organ of the will. He interprets Kant in such a way as to make Kant assert that the mind creates the world of things, instead of merely conditioning it. This perversion of the Kantian doctrine leads Schopenhauer to hold (in contrast to Kant) that the world of space and time is an illusion. Hence he is unable to conserve individuality. and tends to confuse individuality with (temporal and spatial) individuation. Schopenhauer's monism is a mere prejudice against multiplicity, and his will a pure abstraction. Furthermore, he confuses the will with bodily wants and cravings. Schopenhauer exposes himself on every side to such destructive criticism: but while Mr. Laing seizes upon some of his weakest points in his interpretation of Kant, the view of individuality which Schopenhauer represents, and which is more abiding then Schopenhauer, cannot be said to be demolished. η

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Scientia for February, 1917, opens with an article by Gino Loria on the history of imaginary numbers. He takes as his text Kronecker's aphorism "Die ganzen Zahlen hat der liebe Gott gemacht, alles andere ist Menschenwerk." There is no square root of a negative quantity, said the Viga Ganita, for it is not a square, and the mysterious quantities remained an enigma and defied concrete interpretation until a memoir appeared about the end of the eighteenth century, written by an unknown Danish land-surveyor, Caspar Wessel by name. It seems to have suffered a fate like that of Swinburne's Queen Rosamund, of which not a copy was asked for or sold. Mendel's discovery remained unheeded for forty years, but Wessel's was not unearthed until a century after his death. But in this summary is no more room for comment on Loria's charming paper. P. Zeeman writes upon the hypothesis of the immovable ether, describing the experiments of Fizeau, Michelsen and Morley, Eichenwald, and himself, and concluding that it seems impossible by any imaginable means to measure absolute velocities. He points the lesson that the most important scientific principles are the results of boldly generalized experiments. J. R. Carracido discusses the foundations of biochemistry, and the lines research must follow in the pursuit of organic synthesis. He does not see why success should not be reaped before the end of the century, and success it will be, even if limited to the synthesis of the most rudimentary form of living matter.

The number for March, 1917, contains an admirable article by Gaston Milhaud, in which he attacks the very difficult problem as to the extent to which Descartes was influenced by Bacon. M. Cantone discusses the present trend of physical research in a paper surveying the work of those whose discoveries have in thirty or forty years revolutionized our outlook on the world of matter. Etienne Rabaud writes on the life and death of species. An analysis of the current doctrine of "means of defense" prepares for the question as to how it is that species persist in spite of the daily hecatombs of individuals. γ

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In *Scientia* for April, 1917, we have, from the pen of Francisco Iñiguez, of the Observatory of Madrid, a slight but interesting sketch of what we know about stellar spectra, their classification, and the light they throw on the subject of the evolution of the stars. We are warned of the limits we must set to our inferences in considering the nebulae, for what we know as yet of these celestial bodies does not justify our indulging in theories on the subject. The author then indicates how we may infer the existence of dark stars, how their evolution still continues, and points out their connection with meteorites and cosmic dust. Etienne Rabaud brings to a close his paper on the life and death of species, of which this second instalment deals with the conditions of the persistence and of the disappearance of species. He finds the affinity of organisms to be the crux of the problem. This leads to the consideration of the conditions of attraction and repulsion, and to a short discussion of parasitism and symbiosis, with the cosmic influences which, often of great complexity, determine the life of a species. The whole forms a graphic picture of the variations of the relative proportion of individuals and species. The slightest change in the conditions of normal life may lead to the disappearance of the last member of a species, or on the contrary the species may thrive and continue to thrive. Species persist, they increase immeasurably in numbers, or their numbers fall off, and they disappear, subject but to the intervention of two sets of influences, affinity and the circumstances that determine their displacements in space. "These modifications, no doubt, sometimes involve other important modifications in the conditions of life; variations may ensue which find their repercussion in the aggregate of the interaction. Thus, linked to one another and to the world from which they come, the life, the transformations, and the death of organisms are functions of their interdependence." γ

BOOK REVIEWS AND NOTES.

DIDEROT'S EARLY PHILOSOPHICAL WORKS. Translated and edited by *Margaret Jourdain.* "The Open Court Classics of Science and Philosophy," No. 4. Chicago and London: The Open Court Publishing Co., 1916. Pp. vi, 246. Price $1.25 or 4s. 6d. net.

Among the documents of the renaissance of the eighteenth century, none are of more interest than the early informal contributions to ethics and philosophy of Diderot, written with much of the incoherence of the epistolary form. They are, as he claims again and again, *Letters*; and they are letters written in a hurry. *The Philosophic Thoughts,* which is the only one of Diderot's works in this selection not in the epistolary form, is said to have been thrown together between Good Friday and Easter Monday of 1746. Yet they are not philosophic journalism, no mechanical transmission of the current philosophical coin of the day. It is for their originality of outlook that they have been closely studied in Germany, while in England there is Lord Morley's study of Diderot in relation to the movement centered in the *Encyclopédie, Diderot and the Encyclopædists.*

This selection includes the *Philosophic Thoughts,* a breviary of eighteenth-century scepticism, a copy of which was found in the possession of the unfortunate La Barre, and in which Diderot appears as a Deist, to whom the argument from design (Thought XX, pp. 56-58) is still of weight: "I am greatly deceived (he writes) if this proof is not well worth the best that has ever issued from the schools." That very argument is very differently treated in the *Letter on the Blind* (p. 109) by Diderot's mouthpiece, the blind mathematician, Nicholas Saunderson, who conjectures a world in its early stages "in a state of ferment," without any vestiges of that "intelligent Being whose wisdom fills you with such wonder and admiration here.... What is our world, but a complex, subject to cycles of change, all of which show a continual tendency to destruction; a rapid succession of beings that appear one by one, flourish and disappear; a merely transitory symmetry and momentary appearance of order?"

In the brilliant passages in which Diderot sketches the probability of evolution he appears as a forerunner of thinkers such as Erasmus Darwin in England and Lamarck in France. Transformism only needed the partial scientific confirmation it received from Lamarck and Geoffroy St. Hilaire in the early decades of the nineteenth century, "to pass from the realm of systematic philosophy into that of scientific controversy."

The *Letter on the Deaf and Dumb,* a criticism addressed to the Abbé Batteaux, author of the *Fine Arts Reduced to a Single Principle,* has its interest as a forerunner of Lessing's *Laokoon,* in esthetics. It also contains the idea of a *muet de convention* (theoretical mute), which is closely paralleled by Condillac's Statue in the *Treatise on the Sensations,* published three years after Diderot's Letter. Condillac's treatment of the idea, however, was far more systematic and detailed than Diderot's, and he did not by his own account owe the suggestion of his statue to Diderot.

Diderot, the most German of French authors, as far as his style is concerned, bears translation well. He has been neglected by translators, however, until this edition, which includes all that is of permanent value in his early works of 1751, the date of the Letter on the *Deaf and Dumb,* excluding the relatively uninteresting *Sceptic's Walk.* µ

THE NEW PHILOSOPHY OF HENRI BERGSON. By *Edouard Le Roy.* Translated from the French by *Vincent Benson, M.A.* New York: Holt. Pp. 235. Price $1.25 net; by mail $1.35.

This interpreter of Bergson's philosophy is also the author of the article "What is a Dogma?" in the body of this issue of *The Monist.* He is particularly fitted for the present task because though not a pupil of Bergson's he had followed much the same trains of thought quite independently so that when he became acquainted with Bergson he recognized in his work, as he himself says, "the striking realization of a presentiment and a desire." That M. Le Roy has comprehensively grasped Bergson's spirit and conclusions so that the present volume furnishes a valuable *prolegomenon* to the study of the famous Frenchman's thought is attested by the following lines in the Preface in which Bergson himself has set the seal of his approval on the task. M. Bergson wrote to M. Le Roy: "Underneath and beyond the method you have caught the *intention* and the *spirit*.... Your study could not be more conscientious or true to the original. As it advances, condensation increases in a marked degree: the reader becomes aware that the explanation is undergoing a progressive involution similar to the involution by which we determine *the reality of Time.* To produce this feeling, much more has been necessary than a close study of my works: it has required deep sympathy of thought, the power, in fact, of rethinking the subject in a personal and original manner. Nowhere is this sympathy more in evidence than in your concluding pages, where in a few words you point out the possibilities of further developments of the doctrine. In this direction I should myself say exactly what you have said." ρ

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